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A Group of Large Sunspots

The series of photographs of sunspots, shown in Plate II, was taken by the Reverend Mr. William M. Kearons at his home in Fall River, Massachusetts, by means of a four-inch refractor, using the lowest magnification. Mr. Kearons is the rector of the St. Luke's Episcopal Church in Fall River, and he and Mrs. Kearons are astronomers only as amateurs. It may be fitting to remark that being an amateur does not indicate that one is inferior as a worker, but rather, on the positive side, that one does a piece of work because of his love and enthusiasm for it. This series of photographs shows both these aspects of work by amateurs. In the first place, the photographs show experience and skill, and, in the second place, the fact that Mr. and Mrs. Kearons observed and took photographs of the sun every clear day, while these spots were visible, indicates ardor and enthusiasm.

A study of these photographs will reveal certain features which might not be noticed in merely a casual glance at them. The scale of the reproduction is somewhat smaller than the original photographs and hence some of the details of the spots themselves are not so clearly presented. The interest in the series lies rather in the position of the spots on the sun's disk on the several dates of the photographs. The dates of the strips are clearly indicated on the plate.

It will be seen, therefore, that the series on the left traces the movement of the spots until they disappear on the west edge of the sun. Then, after a lapse of time sufficient to carry them through a half rotation of the sun on its axis, the series on the right shows the reappearance of the spots on the eastern edge and follows them a second time across the disk until they again disappear. An interesting feature is that near the edge of the sun the spots seem closer to each other than when near the center of the disk. This is due to foreshortening, a natural consequence of the sphericity of the sun.

There is some change in the appearance of the spots between the time of the first appearance and that of the second. However, there is sufficient similarity in the grouping and in the position on the disk to remove any reasonable doubt that all the photographs represent the same spots. We have here fresh evidence of the rotation of the sun on its axis. It is pleasing thus to have an ocular demonstration of this well-known fact.

When Mr. Kearons sent in the first of these photographs he said, "The accompanying photographs are of a large group of sunspots dating from January 4 to 11, 1940. An interesting feature of this group was the rapid development from several scattered spots to two with unusually

SUNSPOTS PHOTOGRAPHED BY THE REVEREND MR. WILLIAM M. KEARONS, FALL RIVER, MASSACHUSETTS

large umbra and many smaller ones surrounding and some spanning from one large spot to the other.

"To us the group appeared as one fairly large spot on December 31, 1939, two or three days distance from east edge; it was not noticed on the two preceding days because of a cloudy sky. On January 1 this spot appeared with two smaller ones below it. On the next day the two small spots had developed into two fairly large groups. No observation was made on the third.

"Each day after the 4th when the large group was first observed, but too late to be photographed, the spots advanced in almost the same formation to the western limb in quite spectacular fashion and were very pronounced even on the 10th, being visible to the unaided eye. Ordinarily a spot so near to the edge is not so easily visible.

"Another interesting feature is the probability of the reappearance of this group in about two weeks because of its density and size."

By a strange coincidence on the very day the photographs came, we received a communication from Mr. Theodore Paine Palmer, Instructor in Mathematics in Rose Polytechnic Institute, Terre Haute, Indiana, as follows: "On January 6, 1940, I saw two sunspots, on my way to breakfast! The atmosphere was so thick with smoke that the sun showed through it only as a pale reddish disk, which I could look at without discomfort. As I walked, I noticed that certain spots had not moved at all, though I walked two blocks. So they could not be specks in the atmosphere, and I do not wear glasses. Ergo, the smoke served as smoked glass might have served, and I saw two sunspots. Ten minutes later the sun was too bright."

Later, word was received that the sunspots were independently observed by the Reverend Donald G. Henning, Rector of Christ Church in St. Paul, Minnesota. Mr. Henning said that on Monday morning, January 8, he was driving to Minneapolis and at one place going towards the sun he saw through the haze the disk of the sun with spots in the upper part on the right. He was so surprised that he stopped his car and got out to get a better view, and to see whether his eyes were really seeing what they seemed to see. The spots were there plainly enough. He sent a rough drawing of their position which agrees well with photographs on that date.

Mr. Kearons' photographs give unmistakable confirmation of these two quite fortuitous visual observations.

After the reappearance of the group Mr. Kearons sent additional photographs and said concerning them: "On January 25 the group fulfilled our expectations by the appearance of the first large spot at 12:14 P.M., E.S.T., as a very slight nick or indentation on the east edge of the sun. This would have escaped our notice had we not been watching closely for it. By 3:30 P.M. on the same day it was far enough advanced on the face of the sun for us to be sure it actually was the spot for which we

watched. On January 26 at 3:35 P.M. the companion appeared on the edge, so slight an indentation that it was not possible to be sure it really was a spot until the next day.

"By the 27th both large spots were in full view with most of the attendant smaller spots composing the group. This group retained practically the same formation from day to day as it passed across the sun's disk. On the 7th of February only the following spot with a few smaller ones was visible on the western edge, the preceding spot having passed from view. The next day the whole group had passed from view.

"What makes this event of great interest was the fact of its returning and recrossing the sun with very little change in form. This is considered as a rather unusual happening."

This group of sunspots attracted attention in England also. Under the heading "Big Sunspots," *Nature* (London) for January 13, 1940, comments concerning them as follows: "A big group of sunspots, originating about January 1 and growing rapidly, crossed the sun's central meridian on January 5 and reached the west limb on January 12. Between January 2 and 4, the area increased ten-fold and measured 1,300 millionths of the sun's hemisphere on the latter date. Still expanding on January 5, the spots were clearly seen with the naked eye through the morning mist. In structure, the group was a typical stream with leader and follower spots and subsidiary spots in between. This group is the largest since last September, and indicates that, although the maximum of the 11-year cycle is past, the sun is still generally active."

Just as the forms were being closed, word was received from Mr. Kearons that the spots had again reappeared. He says, "Tuesday and Wednesday (February 20, 21) were very stormy and cloudy hence we had no opportunity to see the sun. This morning (February 22) at 7:30 we saw a large spot slightly in from the edge which seems to indicate that it might have been on the very edge yesterday afternoon. Its position corresponds with the large spot on the photograph of January 26. In fact that photograph would almost do for this morning's spot. We feel reasonably certain that this is the leader-spot of the same group on its third appearance being in the same position on or about the date it should be due.

"An item of interest suggested by Mr. Kearons is that, assuming that the first appearance of this group occurred on December 29, 1939, its first return on January 25, 1940, was just four days short of being a month and the second return, if on the edge on February 21, is also just four days short of being a month. A comparison of her charts of observation for today and January 25 shows today's spot in a position almost identical with the position of the spot on January 25."

Astronomy*

By FRANK SCHLESINGER

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Astronomy touches the practical side of our lives in very few places, and not many of us have acquaintance with more than the most elementary facts concerning the skies. This was not always so; if we go back only a few centuries in the history of mankind we come to a time when a knowledge of the stars was forced upon man, and when a close acquaintance with the apparent motions of heavenly bodies formed an indispensable part of his equipment for his contests with Nature. Watches and clocks were invented yesterday, and even the rudest devices for keeping account of the lapse of hours were contrived comparatively recently. Long before these there were the sun by day and the moon and stars by night from which to tell the time, and very well do these serve this purpose for those who know their habits. In cloudy weather some other expedient was needed, as then only the time of sunrise and sunset could be read in the sky. For those who lived near the sea a supplementary timepiece was afforded by the tides, whose regular recurrence and whose relation to the moon and to the sun must have been recognized by the first of intelligent men.

To measure longer intervals than the day and its subdivisions, the month and the year were adopted, depending respectively upon the apparent motions of the moon and the sun. The month is not of much natural importance; but it was needed as a step intermediate in length between the day and the year. The latter, on the other hand, is closely interwoven with men's activities. It is and always has been essential to be able to tell how much of the year has elapsed. Printed helps to do this are now as common as clocks, but in ancient times the sky was the only calendar. In its apparent eastward motion among the stars, the great brightness of the sun masks the feeble light of stars in its path and so makes them invisible for a portion of the year. If we watch the eastern sky in the early mornings, we can tell, with an uncertainty of only a day or two, when a bright star first emerges from the dazzling rays of the sun. This is called the star's heliacal rising. Somewhat less than a year later the star is again lost in the rays of the sun in the western sky and is said to be at its heliacal setting. These risings and settings occur accurately at the same time of the year for any particular star. Thus the farmer would learn, out of his own experience, or more likely from instructions that he received from his father, to time the planting of various crops with these heliacal risings and settings of bright stars.

*A lecture delivered on January 16, 1940, at Yale University under the auspices of the Gamma Alpha fraternity. It is to form a chapter in a book "The History of the Sciences" to be published by the Yale University Press.

In these and other ways the attention of men of all races was early directed to astronomy. Some knowledge of the science grew up among them before they had acquired the art of recording their doings in any kind of writing, for the very earliest of known records show that astronomy had already made some progress. The most remarkable examples of this are found in China, in India, and especially in Chaldea. Eclipses were predicted many centuries ago, probably at least as early as 2000 B.C., or fifteen centuries before the Golden Era in Greece. The ability to make such predictions shows an astonishing degree of proficiency; to be able to do this even now is popularly regarded as a high achievement of the human intellect. But we must explain that whereas eclipses can now be predicted with an error of at most two or three seconds, the ancients had to be content with merely indicating the day upon which such an event was to be expected. Furthermore, the modern astronomer makes these predictions with a complete knowledge of their cause, starting from the known orbits of the moon and the earth. The ancient predictions are on a very different plane. They were made by rule of thumb, through the discovery of what the Chaldeans called the *saros*, a period of a little over eighteen years, within which eclipses repeat themselves in the same order as in the preceding *saros*, but ten or eleven days later in the year.

Any one eclipse of the moon is visible from about three-fifths of the earth's surface. A prediction made according to the *saros* is almost invariably fulfilled, but we know that this is so only because we can now get reports from every part of the globe. To the ancients, word could come from a very limited area, and thus about one-third of the predictions of lunar eclipses apparently failed. With solar eclipses this difficulty was much greater, as any one of these can be seen on the average from only one-fifth the earth's surface, and thus only occasionally would the prediction of a solar eclipse be verified. These missing eclipses must have increased several-fold the difficulty of discovering the *saros* and make the achievement all the more noteworthy. It is probable that the ancients noticed that after the lapse of three of these periods (about fifty-four years and one month), both solar and lunar eclipses recur under nearly the same circumstances and are visible from about the same area. In any case, it is inconceivable that the *saros* could have been discovered otherwise than from recorded data extending over many years. It is also unlikely that it was independently detected among three or four different peoples. Probably the discovery was made by the Chaldeans and knowledge of it spread eastward to India and China, and westward to Egypt and finally to Greece. We cannot help wondering how far astronomy had progressed in Chaldea and whether the people who had discovered the *saros* may not have made other equally striking advances, the record of which has not come down to us.

The restless and speculative character of the Greek mind showed itself very early in its astronomy, and it is the Greeks who must be accounted

the founders of the subject as a true science. To cite only one example: those who preceded the Greeks, for all their success with the prediction of eclipses, do not seem to have been aware that it is the moon that causes a solar eclipse. This idea was advanced by Thales of Miletus (about 640 to 546 B.C.), one of the founders of the Ionian School. From this he was naturally led to conclude that the moon is a dark body and that we see it only because the sun is shining upon it; and from this again he went to the true explanation of the phases of the moon.

Anaximander (611 to 545 B.C.), a pupil of Thales, held that the sun and the moon are much larger bodies than the earth, and thus gave the first intimation of their great distances. Though grossly in error as to the size of the moon, this astronomer must be regarded as the one who took the first of the many steps by which the earth has finally been put into its proper place in the universe. Until this suggestion was made, the earth had always been considered as being in every sense by far the most important part of the universe.

There is a popular impression that it was Columbus who first proved that the earth is round. The fact is that all men of learning knew this to be so many centuries before 1492. All the later Greek philosophers held to this belief, and they had good reason for doing so. After Alexandria was founded in 332 B.C. (and even long before) journeys between Greece and Egypt were frequently made, especially by those who wished to consult the splendid library at Alexandria. It was noticed that in going towards Egypt southern constellations gradually appeared higher in the sky, and that stars that had been barely or not at all visible in Greece were now high above the southern horizon. To such excellent geometers as they were there could be only one explanation, namely, that they had traveled over a curved surface. Again, they knew that eclipses of the moon were caused by this body entering the shadow of the earth; they noticed the circular edge of this shadow upon the half-eclipsed moon and their geometry again told them that only a sphere could always cast a circular shadow. So as early as about 500 B.C. Pythagoras and his school pictured the earth as a sphere encased on all sides by air, and suspended without tangible support in the center of the universe; a very great step in the history of man's progress.

A century later (about 420 B.C.) an equally important advance was made by Philolaus, a member of the same school. Until then it was supposed that all the heavenly bodies, including the stars, revolve daily about a fixed earth. Philolaus saw that this motion might be only apparent; accordingly he supposed that all the heavenly bodies, including the earth, revolve around a common center of the universe; that the sun and planets have periods of nearly a year and thus their wanderings among the stars are accounted for; and that the moon has a similar period of one month. But the earth was supposed to revolve in one day around the same center, not very far from it and keeping the same side always turned towards the center.

The last step of all in this sequence was taken by Heracleides about 370 B.C. For Philolaus' revolution of the earth near the universal center, he substituted a simple *rotation* around its own axis.

More important than any single discovery or hypothesis made by the Greeks was their initiation of a certain scientific method which even now is one of the most powerful tools of the investigator. Advances are to be made by setting up *working hypotheses* and confronting them with observational data, modifying or rejecting them as new facts may indicate. It was Plato (427 to 347 B.C.) who urged his followers to try to represent the motions of celestial objects by means of circles and spheres, and it was his pupil Eudoxus (408 to 355 B.C.) who first put this method into effect. He probably did not know and certainly made no use of the notion of a rotating earth that had been developed by the followers of Pythagoras. He imagined the sun, moon, and planets to be fixed in spheres which revolve within others, all in turn revolving around the earth. By using no less than twenty-seven such spheres he was able to represent well all known motion with the exception of those of Mars and Venus. A few years later these defects were cured by Calippus with the use of six additional spheres. The theories of Eudoxus and Calippus were accepted by the great Aristotle (384 to 322 B.C.), who again introduced additional spheres, bringing their number up to fifty-five. Clumsy as these systems appear to us now, they are noteworthy and praiseworthy as being the first attempts to represent celestial motions mathematically.

Aristotle's contributions to all forms of knowledge were very great and no philosopher has ever lived whose word was more respected. For this reason it amounted to a catastrophe (one indeed that was later to cost some men their lives) that he did not adopt the ideas of the Pythagoreans, but held instead to a fixed earth about which even the stars revolve. And so the truths that Greek genius had discovered were again buried and remained unheeded for nearly twenty centuries, until the labors of Copernicus, Tycho, Galileo, and Kepler finally revealed the true system of the world.

The first astronomer who suffered from the weight of Aristotle's contrary opinion was Aristarchus of Samos (about 265 B.C.). His earliest contribution to the science was a measurement of the relative distances of the sun and the moon from the earth. This he did by estimating when the disk of the moon is exactly at first or third quarter and then measuring the angle between the sun and the moon. In this way he found the sun to be about twenty times as distant as the moon. In reality it is four hundred times as distant. Nevertheless this was a great feat; it was the first attempt to measure the distances of celestial bodies on geometrical principles, and it gave the first solid intimation of the great distance of the sun and therefore of its great size. With this knowledge gained, a study of the relative sizes of the moon and of the earth's shadow during an eclipse would lead to the conclusion that the sun must be much larger

than the earth. To Aristarchus it seemed more reasonable to suppose that the earth attends the sun, rather than that this great and brilliant orb should attend the earth. And so he not only returned to the rotating earth of the later Pythagoreans but went so far as to maintain that the *annual* motion of the sun is only apparent, being really due to the revolution of the earth around it. In this conception Greek cosmogony reached high-water mark. So far as it goes, it presents a picture of the solar system that differs in no wise from that which modern astronomy has proved to be the true one.

The idea that the earth is a sphere had long been an integral part of Greek astronomy. About 200 B.C. an attempt to measure the radius of this sphere was made by Eratosthenes (276 to 196 B.C.). He had been informed that on midsummer day the sun shone directly down deep wells at Syene in Egypt (now the modern city of Assuan, the site of the great dam for the upper waters of the Nile). The sun must therefore have passed directly overhead. At noon of the same day he measured the zenith distance of the sun at Alexandria and found it to be a little over seven degrees, or one-fiftieth part of an entire circumference. This, as we should now express it, is the difference in latitude between the two places. Their linear distance apart was roughly known, chiefly through pacing, to be about 5000 stadia, and as they are about north and south of each other, Eratosthenes concluded that the whole circumference must be 250,000 stadia. Just how close an estimate this is we do not know because there is some doubt as to the length of the stadium employed. According to one value, Eratosthenes' result is almost correct; according to another, it gives dimensions for the earth which are 20 per cent too large. In any case, the experiment yielded a much closer knowledge of the size of the earth than had hitherto been available. This measurement may be regarded as the beginning of the science of geodesy, and it is interesting to note that the method is essentially the same as that of today. Nowadays, however, geodesists measure distances with an error of only one part in a million, and differences of latitude can be ascertained with an error of one-tenth of a second of arc.

If we were to set down in order the names of the astronomers in all ages who have contributed most to the science surely that of Hipparchus would appear very near the top, perhaps not lower than second. Of this great man we know practically nothing but what concerns his work, and that has come down to us very indirectly through the word of men who lived three centuries after his time. He was probably born at Nicaea in Bithynia, but we do not know when, nor do we know when or where he died. Records of observations made by him on the Island of Rhodes between 146 and 126 B.C. are the only clues to the place and the time in which he lived.

Hipparchus proved himself a master of all phases of astronomy, and it is difficult to say which of them owes most to him. In the first place, he improved the construction and design of astronomical instruments,

the chief of which were astrolabes (both plane and spherical), a combination of graduated circles provided with sights similar to those still employed on rifles. This enabled him to determine the places of objects in the sky with an accuracy which was in advance of anything that had been attained before, and over which no considerable improvement was made until seventeen centuries later. This advance in accuracy of observation was the basis of Hipparchus' success. With his improved instruments he determined anew the movements of the sun, the moon, and the planets. In discussing these observations, or reducing them as we should now phrase it, he felt the need of a new kind of geometry suitable to the sphere. For this purpose he proceeded to develop the subject of spherical trigonometry, and this feat alone places him high among great mathematicians. Next he was led, as Eudoxus and others had been before him, to represent the observed motions in the sky by mathematical devices, and here again he succeeded nobly.

The most important discovery made by Hipparchus, and perhaps the one that arouses the most admiration on the part of modern astronomers, is that of the *precession of the equinoxes*. This is the motion of the axis of the earth in space. If this axis is prolonged to the sky the piercing point is *the pole of the heavens*, around which all the stars seem to revolve in their nightly courses. Hipparchus' discovery consisted in showing that the pole of the heavens is not always the same, but that it moves slowly among the stars, describing a vast circle and coming back to its starting point after about 36,000 years. This is the value he assigned, now known to be about 40 per cent in excess of the truth. So slow is this motion that it would have been impossible for an astronomer, provided with such instruments as Hipparchus had, to detect it within the span of one man's years of activity. He discovered this motion by comparing his own observations with those made by Timocharis a century and a half earlier. This is the first example of a kind of coöperation of which the history of astronomy now has many to offer, a coöperation between the present and the past. Many of the phenomena of the skies unfold so slowly that it is hopeless to try to detect them, much less to measure them, in a single generation. Thus many an astronomer has been called upon to make observations which he knows very well cannot bear fruit until long after his death. In this instance it was Hipparchus who was permitted to pluck the fruit. But he more than repaid the debt: for many years after his death, down indeed to the eighteenth century, his own observations were appealed to again and again and formed the basis of later contributions to our knowledge.

Long before the time of Hipparchus the Chaldeans had fixed the length of the year at $365\frac{1}{4}$ days, which is only eleven minutes too long. All the nations that came into contact with the Chaldeans borrowed much of their astronomy, and among other things adopted this value of the year. Hipparchus was the first to learn that it needed a slight correction, which he placed at six minutes, leaving his year still five minutes

too long. This outstanding error in the length of the year accounts for Hipparchus' error in his determination of the rate of precession. The error is not a great one, especially when we consider the character of the observations from which it was discovered; if, however, Hipparchus had used the true length of the year he would have derived a value for the precession that is almost exact.

The appearance of new stars, objects that blaze forth suddenly and temporarily where no stars were visible before, has always played an important part in stimulating researches in astronomy. One of these rare objects appeared in 134 B.C. and was observed by Hipparchus. It led him to reflect on the possibility of the temporary character of other stars, and this gave rise to the first catalog of stars. Hipparchus measured the positions of 1080 stars and in addition gave an indication of their brightness by assigning them to six magnitudes, the brightest being called the first magnitude, and the faintest the sixth. This method for recording the brightness of stars, modified to meet the necessities of modern accuracy, has persisted to our own times.

In the problem of representing the motions of the sun and the moon geometrically, Hipparchus discarded the *spheres* of Eudoxus and adopted instead a much simpler system of *circles,* a device that had been suggested by Apollonius, the famous mathematician. It is to the latter that the invention of epicycles is due, these being circles in which the sun or other body is supposed to revolve, its own center revolving in turn in the circumference of a second circle, called the deferent. Hipparchus used this device to represent the motions of the sun and the moon. He also invented the eccentric, a circular orbit whose center is not at the earth, but removed from it by a small distance called the eccentricity. In this way he was able to represent fairly well all the motions he had observed, including the inequality that he had discovered in the length of time that the sun remains respectively north and south of the equator. With his better knowledge of the apparent orbits of the sun and the moon, Hipparchus was enabled for the first time to compute eclipses on a strictly mathematical basis, and to predict them with a much higher degree of precision than had hitherto been possible.

After the death of Hipparchus nearly or quite three centuries elapsed before any important addition was made to the science. This brings us to Ptolemy, the last of the Greek astronomers, of whose life we know almost as little as we do of Hipparchus'. He lived in Alexandria in the middle of the second century. His great work is the *Syntaxis,* better known under its Arabian title, the *Almagest.* It is this book that has preserved for us the names and deeds of the earlier Greek astronomers, and especially those of Hipparchus.

Ptolemy's admiration for his great predecessor amounted to reverence and the larger portion of the *Almagest* is taken up with an account of Hipparchus' work and with its continuation. Hipparchus had devoted much time and thought to the representation of the orbits of the sun and

the moon. Though he had industriously observed the planets, he had not attacked seriously the problem of defining their motions. This was the great task to which Ptolemy devoted himself, and it is one that required not only an enormous amount of labor, but mathematical baility of a high order. For it must be remembered that Aristotle's cosmogony, adopted by all but one of his successors and even by Hipparchus, now held the field unchallenged; it is easy to see that the problem of representing the motions of planets by means of circles centering about the earth (when in reality these motions center around the sun) is one of great difficulty. The details of Ptolemy's solution would be out of place here; it must suffice to say that by means of epicycles, deferents, and eccentrics he succeeded in representing well the observed positions of all celestial bodies. The greatest differences between his theory and acutal observations amounted to 11′, or to about one-third the angular diameter of the moon.

The *Almagest* refers to numerous observations that Ptolemy himself had made. From his observations of the stars, he states that he confirms Hipparchus' value of the precession, which is now known to be 40 per cent too large. As Ptolemy had the benefit of three additional centuries over which the observations extend, it seems curious at first sight that this inaccuracy should have escaped him. Some writers have gone so far as to suggest that in spite of his statements to the contrary, Ptolemy did not make original observations, but merely brought Hipparchus' observations up to his date by applying the erroneous value of the precession. More than a century ago Laplace cleared Ptolemy of this suspicion, but it still persists in some quarters. The explanation is that with such data as the ancients possessed the amount of precession that is deduced depends upon the length of the year that is adopted. Now Ptolemy did not attempt to determine the latter quantity but simply adopted Hipparchus' value, which as we have already said, is about five minutes too long. These two errors balance each other. Under these circumstances, had Ptolemy derived a value of the precession that differed essentially from that of Hipparchus, then only would there have been any occasion for suspicion.

Although three centuries separate Ptolemy from his predecessor, a much longer interval elapses before the advent of a worthy successor. After his death we hear no more of the cultivation of astronomy in Europe or by European peoples, until the Dark Ages are past and the revival of learning in Europe is in full swing. In the interim we must look to Asia for the little progress that was made. A survey like the present can make no attempt at anything like completeness, and we shall lose nothing in the continuity of the narration by passing over the labors of all who came between Ptolemy in the second century and Copernicus in the sixteenth.

Niklas Koppernigk, or Nicolaus Copernicus in the Latin form, was born at Thorn in Poland in 1473, in a house that is still proudly pre-

served. His father was a merchant of unusual attainments and his uncle was Bishop of Ermland. They taught the boy Greek and Latin and afterwards sent him to the University of Cracow. Here he developed strong mathematical tastes and became proficient in the classics. From the university he returned to his uncle's diocese in Prussia and spent most of his remaining years in the city of Frauenburg, where he died in 1543 at the age of seventy.

Copernicus' contributions to astronomy are wholly on the theoretical side. He constructed some astronomical instruments and made a few observations with them; but in these he was not very happy, falling short of the standard of precision set by Hipparchus so many years before. His fame rests upon his *De Revolutionibus Orbium Caelestium*, published just before his death, by far the most important work in astronomy since the *Almagest*. The thesis of this book is the simplification that is brought about by assuming that the daily motions of the stars and all other celestial objects are due to a rotation of the earth on its axis; and that the annual motion of the sun is only apparent, being due to an orbital revolution of the earth around that body. In other words, he supposed the sun and the stars to be stationary, and that all the planets, including the earth, revolve around the sun, the moon alone attending the earth.

It was only on the score of simplicity that this scheme could be urged in that age. The facts presented by the senses seemed to be against it, the authority of Aristotle was against it, the *Almagest* was against it. Most important of all, men of both creeds in Europe (for the Reformation had already begun) declared the new theory to be contrary to scripture. That it did introduce a great simplification in the mathematical representation of observed motions there could be no denying. It was still necessary to have recourse to the epicycles, deferents, and eccentrics, but their number was smaller than in the Ptolemaic system and there was better accordance between theory and observation.

It cannot be claimed for Copernicus that he established the system that bears his name, and which he so ably sets forth and expounds. Confirmation of this hypothesis had to wait a full century, until the advent of Kepler and Galileo. In the interval still another system of planetary motions was set up by Tycho Brahé.

Tycho was born in 1546 (three years after the death of Copernicus) at Knudsturp, then in Danish territory, now in the extreme southern end of Sweden. His father was a nobleman of some consequence and Tycho was the eldest son. Accordingly he was sent to the Universities of Copenhagen and Leipzig, with the view to preparing him for dealing with affairs of state. But Tycho was emphatically a man with a personality. He did not take kindly to this plan and instead devoted much of his university activity to the study (secretly for the most part) of mathematics and astronomy. At the age of twenty he lost his nose in a duel and thereafter wore an artificial one of brass. Some years later he managed to strain relations with his family by contracting a marriage that

they thought beneath him. At the age of thirty his reputation as an astronomer was such that King Frederick II of Denmark built for him a magnificent observatory on Hveen, a small island in the Sound just west of the Swedish coast, now belonging to Sweden. This observatory, Uraniborg, the ruins of which are carefully preserved and pictures of which have come down to us, was a veritable castle. Here Tycho spent twenty stormy years, but very fruitful ones nevertheless. His royal patron having died, and the new king (or his ministers) having less vision or possibly less patience than the old, Tycho finally deserted his beloved Uraniborg in 1597. Two years later he found a second admiring patron in Emperor Rudolph II, who installed him and his instruments fittingly in a castle near Prague. But only two years remained to him in which to enjoy his new home; there he died in 1601 at the age of fifty-five.

Tycho's great achievement was the improvement that he made in methods of observing. His instruments were built on so generous a scale that some of his graduated circles showed a mark for each minute of arc, and this small quantity was about equal to the average error of one of his observations. It was not only in the actual observing that Tycho excelled those who had preceded him; he corrected his observations for all known errors (refraction, for example) with great care and thoroughness. He seems to have been the first to recognize the advantage of repeating observations several times in order to get rid, as far as possible, of accidental errors. Finally, in their number and continuity his observations are altogether unprecedented.

Tycho saw the great merits of the Copernican system and also felt the weight of the objections against it. The heaviest of these was furnished by his own observations. Aristarchus had found that the sun is about twenty times as far away as the moon; Ptolemy had determined the distance of the moon to be about sixty times the radius of the earth, and so the sun's distance must be more than one thousand such radii, a quantity that even to us today is unthinkably great. If the earth is really traveling around the sun, then when we observe the same stars at opposite times of the year our point of view has changed by twice this great distance, and there should be a perspective displacement called *parallax*. But Tycho's accurate observations, some of them made for this express purpose, showed not the slightest trace of such parallax, and he was certain that no such effect as great as 1′ could have escaped him. In other words, he was confronted with the alternative of supposing that the earth is stationary, or that the distances of stars were more than 7000 times the distance of the sun, and therefore more than 8,000,000 times the radius of the earth. The latter seemed to him (and to many competent contemporaries and successors) to be absurd.

To meet this objection as well as others of a physical character to which the Copernican system seemed open, he modified the latter as follows: the sun and the moon are supposed to revolve around a fixed earth and the planets to revolve around the sun. Except for the parallax of

the stars this system is geometrically the same as the Copernican; that is, the relative positions of any two objects at any specified time are the same in the one as in the other.

Tycho's system has been severely dealt with by later writers, who refer to it with such adjectives as "grotesque" and "unfortunate." Viewed, however, from the standpoint of contemporary knowledge, the system had something in its favor. It represents well the observed facts of that day, and this is the real test of a useful hypothesis. As science progresses, less and less stress is laid upon so-called laws of nature. We have learned to recognize that each advance is to be regarded as a closer approximation to the truth, and that no advance can quite bring us to absolute exactness. The first thing then that we expect of systems, hypotheses, and theories is that they shall conform to the facts.

Whatever our judgment as to the merits of Tycho's cosmogony, his fame as one of the greatest of astronomers is assured by his practical work. He reviewed and improved all the fundamental data of the science, with the single notable exception of the distance of the sun. Like Hipparchus he constructed a catalog containing the positions of about one thousand stars and this work remained the authority in its field until a century later. Like Hipparchus, too, Tycho had the good fortune in 1572 to witness the outburst of a new star; indeed, this occurrence is said to have decided him to devote his life to astronomy. He was also lucky in the number of bright comets that appeared in his time, his observations of these objects forming a precious heritage to succeeding astronomers. He was the first to show that comets are not in our atmosphere but far beyond. Comparing his observed places of the planets with the accepted tables he noticed discrepancies which he recognized were due to the imperfection of these tables. Perhaps if he had not died so comparatively young he might have attempted to reconstruct these tables, but that task was reserved for his young friend and pupil, Kepler, whose work was destined to put the science on a new footing.

John Kepler must be ranked with Hipparchus, Galileo, and Newton as a leader in the history of the science. Laplace speaks of him as one of those rare spirits that nature gives to science from time to time for the purpose of causing the work of many centuries to blossom. He was born at Weil in Wurtemberg in 1571 and was accordingly twenty-five years younger than Tycho. He got his schooling at Maulbronn and Tubingen and taught for a time at Gratz. A Protestant in a community predominantly Catholic, at a time when religious feeling was at its height, he fled to Hungary, finally accepting in 1600 Tycho's invitation to join him at Prague. On the death of Tycho the next year Kepler succeeded him as astronomer to the Emperor and came into possession of all his wealth of observations; he was the one man on earth to whom this inheritance should have come. Kepler did not get Tycho's instruments, but perhaps this was fortunate rather than otherwise, for their possession might have taken away from him and from the world some of the precious years

that he devoted to theoretical work. Even as it was, the undisturbed quiet and freedom from petty tasks that are so essential to sustained intellectual effort were only rarely his. The religious unrest that marked the time laid a heavy hand upon him and several times compelled him to seek a new home. Throughout all the later years of his life he was confronted with the problem of earning a living without having to neglect his real work. More than once he was driven to the practice of astrology, an expedient that he must cordially have hated, whatever his real opinion may have been as to the validity of astrological prediction. His life was cut short as the result of a long and severe journey on horseback to Regensburg, whither he had come to try to collect his arrears in salary as astronomer to the Emperor. He died in 1630, at the age of fifty-nine.

Kepler began his work on Tycho's observations of Mars, first attempting to represent them, as all his predecessors had done, by means of the timeworn epicycles, deferents, and eccentrics. The best he could do left considerable discrepancy between his theory and the observations, the differences running as high as 8′. He knew that so large an error in Tycho's work was not admissible and so he undertook, as he said, out of those 8′ to reconstruct the universe. It was in this way that he was finally led to what is known as Kepler's first law, that the orbits of the planets are ellipses with the sun at one focus. We can imagine his joy when he found that with the elliptic orbit the large discrepancies melted away not only in the case of Mars but for the other planets as well.

It had been long recognized that the angular speed of a planet around the sun is not uniform. Kepler now tried to discover the law of this motion when the orbit is an ellipse. Again after long and patient trial he discovered his second law, that the line joining a particular planet with the sun sweeps over equal areas in equal times. These two laws were published in 1609 in his book, *Commentaries on the Motion of Mars*. Ten years later he announced the third of his laws in a book entitled the *Harmony of the World*. This law relates the periods of the various planets to their distances from the sun; he found that the squares of the former are proportionate to the cubes of the latter.

It is difficult to conceive how startlingly new these discoveries were. The history of almost every great advance reveals that many men have contributed their quota, though it is given to one man to set the capstone that finishes the structure. In this respect Kepler's work is almost unique; perhaps no other like edifice in the whole history of science is so completely the product of one hand. That planetary orbits must be circular was taken for granted by everyone from Plato down even to Copernicus, who had no hesitation in throwing over accepted notions in other respects. It was not that the ellipse and similar curves were unfamiliar, it was simply because circularity was as much a matter of scientific creed then as, for example, conservation of energy is now.

Kepler's laws were by no means his only contributions to astronomy, but to turn to his other work is an anticlimax, important as these re-

searches are. Starting with his three laws, he derived anew the motions of all the bodies in the sky and set down the rules and data that made it an easy matter to compute the position of any of them long in advance. This work was issued near the end of his life as the *Rudolphine Tables*, in honor of his and Tycho's imperial patron. They held the field for a century, until the application of the telescope afforded data more accurate than Tycho's and made possible the calculation of orbits with greater precision. He was the first to show that the sun was more distant than had been supposed by every astronomer since Hipparchus, but Kepler's own estimate still fell far short of the truth. He was among the first to notice the corona, the beautiful light that surrounds a totally eclipsed sun, and the first to assign its origin correctly as a faint but permanent appendage of the sun. He wrote an important work on comets, bodies that he mistakenly supposed to move through the solar system in straight lines. He explained the fact that the tails of comets always point away from the sun by supposing that the light of the sun forms the tail by driving particles out of the head, an explanation that is surprisingly like the one that has only recently come into favor.

Very few investigators in describing their work lead us over the path that they have actually followed. It almost always happens that once having reached the goal, the new point of view reveals short cuts that had escaped attention, and it is these short cuts that most investigators like to describe. Kepler's books are delightful exceptions to this; he frankly records his failures as fully as his successes and lets us see all the mental processes by which he finally arrives at his results. It is encouraging to the student to see how many false trails so great a man had to explore before finding the road to his destination.

Few men have had the patience and perseverance to go through so great a mass of routine computing, not only without the aid of others but without any of the mechanical helps that are now so familiar. News of Napier's invention of logarithms, together with the first logarithmic tables, reached him only after most of his work was done; and so he benefited little from a device which, as Kepler himself said, triples the life of an astronomer.

Up to now astronomy had been on an empirical basis and could not be otherwise until the science of physics should be as thoroughly recast as astronomy had now been through Kepler's laws. The revolution in physics did not tarry long; indeed, it was begun in Kepler's time, by Galileo, born at Pisa in Italy in 1564, seven years before the birth of Kepler, and eighteen years after that of Tycho. It is something of a coincidence that three astronomers so great as these should have lived at the same time. Galileo never met either Tycho or Kepler, but it is pleasant to know that with the latter he exchanged letters and that the two held each other in high friendship. Galileo survived Kepler by twelve years, dying early in 1642 in his seventy-eighth year.

The story of Galileo, and especially of the later years of his life when

the dreadful Inquisition all but crushed him, occupies an important place in the history of intellectual development and intellectual freedom, and is too well known to warrant repetition here. Nor do we need to recount his work regarding the laws of motion, which will be duly emphasized in the lecture on Physics in this series.

It is not known who first devised a telescope; perhaps it was Roger Bacon in the thirteenth century, a pioneer long in advance of his time in several matters. Telescopes are described by Digges in England and Porta in Italy in the middle of the sixteenth century. Certainly it was not Galileo who made the invention; he heard in 1609 that Lippershey and other Dutchmen had the year previous constructed a combination of glasses that made distant objects look near. Acting upon this hint he succeeded in working out the manner in which this could be done, and soon constructed a telescope that magnified thirty diameters. Galileo does not even seem to have been the first to point the telescope to the skies, though he was the first to understand what he saw there. Discovery after discovery of the most startling character followed in rapid succession. He found the moon to be largely covered with rugged mountains (thus confirming the guess that Anaxagoras had made twenty centuries before) and succeeded in correctly measuring the heights of some of them. Similarly he confirmed an equally old conjecture of Democritus when his telescope showed that the Milky Way is made up of a myriad of faint stars. Next, early in 1610, he discovered four moons of Jupiter, revolving in from two to sixteen days. No argument bearing upon the validity of the Copernican system had so great an effect upon public opinion as this discovery. The opponents of this system had maintained that if the earth were truly revolving around the sun then the moon would soon be left behind the earth. But here was Jupiter attended not merely by one moon but by four, and all of them managing somehow to keep up with Jupiter in his twelve-year journey around the heavens.

There still remained the argument that Tycho had emphasized, the absence of measurable parallax in the positions of stars. Reflecting upon this circumstance, Galileo hit upon a method for testing the presence of parallax that is much more delicate than those used up to that time. Tycho, for example, had attempted to find such a parallax substantially from changes in a star's altitude at opposite seasons of the year. Galileo's telescope revealed faint stars in the neighborhood of every bright one, and these he inferred must on the average be farther away. Now all the stars in the field of view would be affected by some parallactic shift, but the nearer ones to a greater extent than the distant; as such relative or *differential* shifts can be detected with much greater facility than the *absolute* changes sought by Tycho and others, here was a method that held out some promise of success. This is essentially the method employed today, but so distant do the stars prove to be, and so small their parallax, that more than two centuries elapsed after Galileo's proposal was made before the art of measuring had reached the degree of perfec-

tion necessary to reveal this perspective displacement for even the nearest of the stars.

Galileo shares with several others the honor of discovering spots on the sun. These objects seemed greatly to offend the sense of propriety of some of his contemporaries, and attempts were made to keep the sun immaculate by maintaining that the so-called spots are planets revolving near the sun's surface. But Galileo showed that they are on the sun's surface and that their motions could only be explained by a rotation of the sun around an axis in a little less than a month.

Galileo's application of the telescope to astronomy was the occasion of truly epoch-making revelations. Nevertheless, they do not constitute his principal claims to the gratitude of astronomers. Most of these discoveries would probably have been made soon after by others, though few would have seen so readily or explained so clearly their philosophical import. Of greater consequence than these discoveries was the part he played in establishing the true system of the world, a far more important one than that played by Copernicus himself; and of still greater consequence was his formulation and experimental proof of the laws of motion, a work that prepared the way for Newton and for the discovery of the law of gravitation.

Isaac Newton was born in 1642, the year of Galileo's death. He was the posthumous and only child of a farmer at Woolsthorpe, a small village about one hundred miles north of London. Like all his great predecessors in modern astronomy (Copernicus, Tycho, Kepler, and Galileo) he had the advantage of university training, graduating from Trinity College, Cambridge, in 1665 and remaining connected with that institution until 1701. During this period he prepared and published (1687) his great work on the *Philosophiae Naturalis Principia Mathematica,* commonly referred to as the *Principia.* In 1699 he was appointed Master of the Mint, a responsible public office that he held until his death in 1727 in his eighty-fifth year. To him more than to most was given that quiet and uneventful career that great philosophers need and deserve.

In 1666 Newton was driven from Cambridge by the plague that had spread from London, and he spent some months at his mother's home at Woolsthorpe. There he began his researches into the nature of gravitation. It must not be supposed that Newton discovered the existence of gravitation. Many writers refer to such a force and even the Greeks had some vague notions about it. In Newton's own day and especially by his own countrymen, we find many references to it. It was Newton who found out how gravitation varies with distance and who showed that the familiar phenomena of falling bodies at the surface of the earth were manifestations of a force that pervades the universe.

Wherever we go upon the earth, even to the tops of the highest mountains, all objects tend towards the center of the earth with apparently undiminished vigor. How far up does this tendency go? Can it reach as far as the moon? These were the questions that Newton asked and

answered. Galileo had already shown that uniform motion is as natural as rest, and that deviation from such motion implies a force. If then the moon were relieved of all forces it would leave its orbit and move along a tangent. Consequently, if the earth attracts the moon, then what that attraction really does is to draw the moon out of the tangent into the orbit. As the period and distance of the moon are known it is easy to compute how much the moon falls away from the tangent in one second. Comparing this with the speed of a falling body in our laboratories, Newton found the ratio of these two speeds to be about as one to 3600. As the moon is sixty times as far from the center of the earth as we are, this implies a force that decreases with the square of the distance.

Is there a force residing in the sun, keeping the planets in their orbits, similar to the one that keeps the moon revolving around the earth? The answer to this question is to be found in Kepler's laws. Newton showed that the second law (equal areas in equal times) implies a force directed towards the sun; that the first law (that orbits are ellipses with the sun at one focus) is a consequence of the inverse-square law of attraction; and finally that if the same law holds in going from planet to planet, then the periods and distances of the planets must be related as required by Kepler's third law, modified a little to allow for the masses of the planets, which are not quite negligible compared with that of the sun, as Kepler had tacitly assumed. Newton was now able to announce the law of gravitation in its most general form: *every particle of matter in the universe attracts every other particle with a force that varies inversely as the square of their distance apart, and directly as the masses of the two particles.*

A good deal of the progress of astronomy since this law was announced has been concerned with its application to the solar system in all its ramifications. Newton himself pointed out many of its chief consequences. He found in it the explanation of the tides. The moon attracts a particle at the center of the earth less than she does the ocean on the nearer side, and more than the ocean on the farther side; and so the water bulges away from the center at both these points. As the earth revolves on its axis every place is thus brought twice daily into regions where the water is high, and twice daily where it is low. The sun raises two similar tides, but as he is so much farther away than the moon, his tides are less than half as great in spite of his much greater mass.

It had already been indicated by pendulum experiments that the earth deviates slightly from a truly spherical form, modern measurements showing that a diameter through the equator exceeds that through the pole by about one part in three hundred. Newton pointed out that this form is a consequence of the earth's daily rotation around the polar axis; and he went on to show that the precession of the equinoxes, discovered so many centuries earlier by Hipparchus, was caused by the attraction of the moon and the sun for this additional matter at the earth's equator.

Up to the second half of the seventeenth century astronomy was fol-

lowed by a few votaries, for the most part widely separated from each other. It is a striking fact, for example, that of the great modern names we have thus far dwelt upon, no two belong to men of the same nationality, and none of them can be said to have founded a school. Now, however, we enter a period when science calls for and attracts many devotees. National academies are created for the promotion of research, regular media are provided for the publication of results, great observatories like Greenwich and Paris are founded and bring together groups of men who are working on similar problems.

Thus far we have been able to adhere to the chronological order of events, for the simple reason that astronomy has been concerned with a single great problem, the plan of the solar system. But hereafter the subject becomes more varied, and in a brief account all that we can hope to do is to reproduce the spirit of astronomical progress. What follows is a set of selections from the history of the subject, made less with a view to their relative importance than because they can be described without recourse to technical details. That some great names and some great deeds are omitted is inevitable.

The application of Newton's law throughout the solar system is a task of incredible difficulty, one that has taxed the powers of such giants as Laplace, Lagrange, Euler, Adams, Delaunay, Hill, Newcomb, and Poincaré. It is of the greatest philosophical importance that this task be completed, for in hardly any other way can we hope to learn what forces other than the gravitation of known bodies are abroad in the universe. Already this method of exclusion has led to important results. It was in this way that Neptune, the outermost planet, was discovered. In 1783 William Herschel, then a professional musician and an amateur astronomer at Bath, England, had discovered Uranus, the first addition to the sun's family of planets since remote antiquity. It was soon learned that after allowing for the attractions of all known bodies the computed path of Uranus did not conform with observation. Leverrier in France and Adams in England independently concluded from these differences that still another planet must exist beyond Uranus, and they were able to say where to look for it. And a new planet, Neptune, was found by Galle in 1846 on the first night he searched.

The motion of the moon furnishes another example of somewhat the same kind. After allowing for the action of all known bodies, two outstanding effects remain; one of these is the existence of the so-called *fluctuations,* apparently erratic in character, for which no explanation is as yet forthcoming. The other is an exceedingly small speeding up of her rate of revolution. In recent years it has been shown that this effect is only apparent and is due to the fact that our timepiece (the earth) is at fault, the friction of the tides on the floors of shallow seas being sufficiently great to slow up the earth's rotation a trifle and thus make the moon appear to run fast.

Still a third effect of the same kind is present. Gravitation alone does

not explain satisfactorily the motion of Mercury, a very slow revolution of the whole orbit at the rate of 40″ a century remaining to be accounted for. We shall refer to this again.

Newton's genius shone with so much brilliance that our eyes are blinded to the lesser lights of his time, lights that would have been far more conspicuous if that great luminary had not been above the horizon. This is particularly true of Edmund Halley (1656 to 1742), a close friend of Newton and his junior by fourteen years. It is doubtful whether Newton could have been induced to prepare the *Principia* without Halley's encouragement and persuasion, and Halley personally bore the expense of printing this work. This man's activities covered an extraordinarily wide field. He made important contributions to terrestrial magnetism, to the explanation of aurora, and to the theory of life insurance; he first predicted the return of a comet (the one that has since borne his name), suggested the first accurate method for determining the distance of the sun, compiled the first catalog of southern stars, and discovered two important perturbations in the motions of the planets and of the moon. But the discovery that we wish to speak of in more detail relates to the *proper motions* of stars, their changes of position with respect to each other. Twenty centuries earlier Hipparchus had constructed a catalog of stars, setting down their positions as accurately as his instruments permitted. Halley in 1718 compared these positions with those made in his own time, and detected motions in several of them. This was the first indication of how important it is to observe the places of stars, and may be considered the beginning of a research which is by no means complete even now, one that has taxed the industry, patience, and ingenuity of all succeeding generations of astronomers. The idea that these motions may be in part only apparent and due to a motion of the sun among the stars occurred to Bradley of Oxford in 1748 and to Mayer of Göttingen in 1760. But the first one to show that this is so from the numerical data was William Herschel (1738 to 1822), who in his first research used only seven stars. He found that their motions indicated that the sun (carrying all his planets with him) is moving swiftly towards the constellation Hercules. This general conclusion has been verified by many later researches dealing with many thousands of stars. According to this view all the stars, including our sun, are moving hither and yon in haphazard directions. Forty years ago a new phase of this subject was revealed by Kapteyn of Holland. He found that after allowing for the motion of the sun, the movements of the stars are not haphazard, but that they can be accounted for by supposing that there are two groups of stars, within each of which the motions are haphazard, but the two are moving through each other. This is Kapteyn's own interpretation; a very different one was suggested by Schwarzschild of Potsdam (1873 to 1916). who supposed that there is only one group of stars, some of which will be found moving in any specified direction, but as a whole showing preference to motions that are parallel to a certain line. These

two interpretations sound radically different, but they lead to nearly the same statistical results, at any rate so nearly the same that we cannot distinguish between them with our present knowledge of proper motions. Perhaps there is no other problem with which astronomers have to deal that is philosophically more important than this, for its solution will tell us whether the so-called universe of stars is really a universe, or whether it is dual in character and possibly in origin.

Such problems as these account for and justify the extraordinary amount of labor that has been put into the determination of star places and therefore their motions. Many catalogs containing accurate positions of thousands of stars have been issued in the past century and a half, beginning with that of Bradley in 1755. Altogether more than one million observations of this kind have been published, all made by the laborious method of observing directly with the eye by means of meridian circles. About fifty years ago a very ambitious plan for the location of several million stars with the aid of photography was undertaken by international coöperation. This work, the *Astrographic Catalog,* was badly interrupted by the Great War, and it is even now far from completed.

The determination of the distances of stars has engaged the attention of astronomers in every age. It has not only been of prime importance in itself, but has yielded unusually interesting by-products. For one thing, no other problem has so constantly spurred astronomers to improve their methods of observing. We have seen that Tycho found stellar distances to be so great that their parallactic shifts were insensible to his unprecedentedly accurate instruments. He would doubtless have been surprised to learn that had his instruments been one hundred times as accurate they could still not have revealed these shifts. But long before astronomers were in possession of instruments of this degree of accuracy they knew in a general way how distant the stars must be. This knowledge came with the realization that the sun is only a star; considerations of the relative brightness of the sun and stars led at once to a proper conception of their distances. Attempts to measure these distances directly followed almost every substantial improvement in methods of observing. James Bradley (1692 to 1762), easily the most accurate observer up to this time, made observations for this purpose, but instead of yielding a parallactic displacement the observations led to the discovery of the *aberration* of light. This is an effect caused by the combination of the earth's motion around the sun, carrying the observer along at the rate of nineteen miles a second, and of the speed of the light from the star, about ten thousand times as great. As a result all stars in the sky are apparently displaced by amounts up to 20″. The detection of a measurable parallax would have afforded direct observational evidence of the motion of the earth around the sun and therefore of the truth of the Copernican system, though, of course, proof of this kind was no longer needed. Curiously enough, this unexpected detection of aberra-

tion furnished equally convincing proof to the same effect, since its existence depends as much upon the motion of the earth as does that of stellar parallax.

Galileo had suggested that the relative positions of a bright and a faint star close together in the same telescopic field might yield evidence of parallax. William Herschel, provided with a reflecting telescope capable of sustaining unusually high magnifying powers, put this suggestion into practice. Like Bradley he failed, and like Bradley he made instead a wholly unexpected discovery of first importance. He had selected pairs of stars under the assumption that the brighter star of the two was nearer than the fainter; but in some of these cases he found that the two were slowly revolving around each other; or in other words, they formed *binary* systems, held together by their mutual attraction. Subsequent study of such pairs, of which thousands are known, shows that Newton's law of gravitation applies to them, and thus extends this law beyond the confines of the solar system.

The invention by Dollond and Bouguer of a new kind of measuring instrument, the *heliometer,* its improvement by Fraunhofer, and above all its skillful use by Bessel of Konigsberg in 1838, yielded the first reliable measurement of a parallax. The method employed is necessarily a very laborious one, so that by the end of the nineteenth century trustworthy distances for only thirty or forty stars had been accumulated. Since then photography with telescopes of great focal length has increased the accuracy of these determinations and has greatly decreased the labor of making them. This work now forms an important part of the observing programs with seven or eight of the most powerful telescopes and has already yielded accurate parallaxes of five thousand stars. In recent years a spectroscopic method for determining stellar distances has been developed by Adams and Kohlschutter at Mount Wilson Observatory. Stars appear to us to be of different magnitude or brightness partly because they are at various distances from us, and partly because their actual or *absolute* brightness differs enormously, some stars sendinging out many million times as much light as others. If we had any way of estimating this absolute magnitude we could say how far any particular star must be away in order that its light should reach us with the intensity that it appears to have. The new method furnishes a means for making this estimate. It is based upon the fact that the intensities of certain lines in stellar spectra are found to vary with absolute brightness; we can now reverse the process and infer the absolute brightness from a study of the relative intensities of these critical lines. This method proves to be more accurate than any other in the great majority of cases, and is also very expeditious, the distances of several thousand stars having already been ascertained in this way.

This latest triumph of the spectroscope is one of a long series. Through the labors of Huggins, Lockyer, Pickering, Vogel, Campbell, and many others, we can now tell from a study of the spectrum of a celes-

tial object what known elements enter into its composition; whether it is gaseous or stellar; if the latter, whether it is single or double (and a surprising number turn out to be double); the rate at which it is approaching or receding from the solar system; its approximate temperature; and now finally, its distance.

The labors of astronomers are for the most part bent to the extension of our knowledge of nature, necessarily with little thought as to the application of any such knowledge to everyday life. There is one noteworthy exception to this: the study of the sun not only possesses all the allurements of so-called pure science, but those who devote themselves to it have the added satisfaction of knowing that such work will in all probability eventually lead to results of material importance. Radiations from the sun of various kinds are the source of nearly all forms of energy available to us and are essential to the maintenance of life on this planet. Additional knowledge concerning the nature of these radiations will doubtless materially enhance the general welfare of mankind. Essential crops throughout vast areas are occasionally afflicted with lean years that bring misery to millions of men. It is not too much to hope that the time will come when such lean years will be predicted and thus make it possible to forestall much of the suffering they entail.

The study of the sun progressed very slowly at first. The detection of sunspots early in the seventeenth century remained an isolated fact for more than two centuries. By that time the spectroscope was turned to the sun and some of the innumerable dark lines in its spectrum were mapped by Fraunhofer. But it was not until 1859 that Kirchhoff and Bunsen found the key to these lines; they showed that most of them are caused by absorption in the very shallow and comparatively cool atmosphere of the sun. Each element has its characteristic lines which appear wherever the absorption takes place, whether in the laboratory, in the sun, or elsewhere. Here then is a means of analyzing the sun in the chemist's sense. It is found that almost all common elements that we find at the surface of the earth are also plentiful in the sun. But many of the lines in the sun have not yet been identified with known elements. They offer a continual challenge to the astronomer and to the physicist.

For two centuries after the discovery of sunspots, no one thought it worth while to observe these evanescent objects systematically, any more than today we should deem it profitable to record in detail the number and size of the clouds in our own atmosphere. But in 1826 Heinrich Schwabe, an apothecary of Dessau began to observe them on every clear day with a small telescope, and continued to do so for nearly half a century. He found that in some years hardly a spot was to be seen, while in others the sun's face was crowded with them. This was the discovery of the sunspot cycle, or period of eleven years. The same cycle has been found to apply to phenomena that have apparently no connection with sunspots. For example, the magnetic needle shows a daily swing about its average position; Lamont found in 1851 that in the years when sun-

spots are plentiful this swing is more pronounced than in other years. Again, aurorae are rare when sunspots are rare. The corona, that pale light visible around the sun only during a total eclipse, changes its shape and general appearance in the same eleven-year cycle, and so on. The nature of the connections between these various phenomena is still unknown; probably no one of them is the direct consequence of any other, but all are effects of the same underlying cause.

Until lately practically nothing was known concerning the nature of sunspots; we are still far from anything like definite knowledge, but at least a clue has been found. A gas heated to incandescence gives a spectrum made up of sharp bright lines. Twenty-five years ago Zeeman found that if the light of this gas is made to pass through a strong magnetic field, these lines are now split into two or three components. The spectrum of sunspots differs in a similar way from that of the general surface of the sun, and Hale at the Mount Wilson Observatory has proved that the cause of this difference is the same, and that sunspots are the seats of strong magnetic fields. Furthermore, the spots are found to occur in pairs with opposite kinds of magnetism, as though the ends of an enormous magnet were protruding from the interior of the sun to its surface. Hale has likewise shown that the whole sun may be regarded as a magnet, being in that respect like the earth, so that a balanced magnetic needle (or compass) on the sun would indicate the north direction just as it does here.

Before the application of photography to the heavens, only a few spiral nebulae were known. In 1900, Keeler, working with the Crossley reflector of the Lick Observatory during the closing months of his all-too-short life, showed that more than 100,000 of these objects were within reach of his telescope. V. M. Slipher, of the Lowell Observatory, with a spectroscope attached to their comparatively small refractor, and making photographs that necessitated accumulated exposures covering several successive nights, showed that these nebulae are all rotating, and that with the exception of the nearest two, they are all apparently speeding away from us with high velocities. A method of estimating the distance of these objects was devised by Hertzsprung; this was by means of the Cepheid variables and the assumption (since fully substantiated) that their real brightness can be inferred from their periods; a comparison between the real brightness and the brightness as seen from the Solar system then yields the distance. Hubble, working with the Mount Wilson 100-inch reflector, succeeded in discovering many Cepheids in the spiral nebulae and in measuring the velocities of the latter. He showed that for the fainter nebulae, their distances could be well estimated from their size and brightness. He then found that their velocities (always away from us) are closely proportionate to their distances. By no means all astronomers believe that these velocities are real; they prefer to regard them as evidence of some cause as yet unknown that occasions shifts in the spectral lines merely similar to those caused by velocities. If they are

true velocities, then it is necessary to asume that the universe is expanding rapidly, speeds up to nearly 50,000 km/sec having been recorded. If that rate were continued backward into the past all these spiral nebulae must have been in the immediate neighborhood of one another only 2,000,000,000 years ago. There are rocks in the earth's surface that are as old as this.

As early as the closing years of the nineteenth century the suspicion began to grow upon astronomers that our own Milky Way was nothing more than one of these innumerable spiral nebulae, but the proof of this is only now in the making. Oort of Leiden has recently shown that the stars in our part of the Milky Way are revolving around a distant center in the manner that such an hypothesis demands. One of the chief objections to this idea is that our Milky Way has seemed to have linear dimensions that would make it by far the largest spiral nebulae of which we have knowledge. This objection has been at least partly removed by Trumpler of the Lick Observatory who showed that hitherto unsuspected obscuring matter in the Milky Way has made us exaggerate its dimensions, since these latter are inferred from the brightness of distant stars; not all of the diminution in brightness is due, we now know, to great distance—some of it arises from the obscuring material. On this new basis our Milky Way still comes out larger than its neighbors among the nebulae, but no longer so much so as to make this a formidable objection.

You will hear fully in the forthcoming lecture on Physics, of the revolution brought about in that science by the theory of Relativity. Here we shall point out briefly its astronomical consequences. Einstein published his special theory of relativity in 1905; this is not of great interest to the astronomer as such. In 1915, however, he published his general theory, which for the first time gives a plausible explanation for gravitation. Even though the Great War was on, this theory caused much excitement in scientific circles. It accounted excellently for the discrepancy in the motion of Mercury as calculated from Newtonian mechanics; and it predicted a deflection in star-light as the beam passed near the edge of the sun. To test this effect observations during a total eclipse of the sun were necessary. Before the war was ended British astronomers organized two expeditions to observe the eclipse of May, 1919, from stations in South America and Africa. Both expeditions were favored with clear skies and both reported deflections close in value to that predicted by Einstein. This result has been confirmed at every suitable eclipse during the past twenty years. A further effect predicted by Einstein, a slight shift toward the red end of the spectrum of light radiated from a heavy body, has also been detected in the sun and particularly in the small and very dense companion to Sirius. Astronomers were as a whole rather slow in accepting so revolutionary a theory, but now, with the accumulation of so much observational evidence in its favor, there are few who do not regard relativity as having been proved.

The early history of astronomy is closely interwoven with that of mathematics and its recent history with that of physics. The astronomer has borrowed almost all his tools from the physicist; the prism, the grating, the photoelectric cell, the interferometer, the photographic plate, even the telescope itself, were all used in the laboratory before they were applied to the sky. But it would be a mistake to regard astronomy as being a branch of physics or of any other science. Astronomy has a distinguished and essential characteristic that it is well to emphasize if we wish to understand the spirit of its history. Many of the facts of the science can be brought out only by observing changes that are going on in the sky, and many of these changes are exasperatingly slow. Some are rapid enough to be detected or well measured in a century or less, while others require thousands of years to run their course. Some of the greatest names in the history of the science are those of men who foresaw the possibility of such changes and who set about making as accurate observations as they could, knowing well that they could not live to see these observations fulfill their purpose, but conscious of the value that their work would take on after the lapse of years. It is this characteristic of his work that is at once the despair and the inspiration of the astronomer. He often has occasion to envy the physicist and the chemist who are able to complete their own experiments. On the other hand, his work is not so easily superseded as theirs; for although it will happen to him as often as to anyone else that later improvements will make his own observations relatively inaccurate, these observations may still continue to be useful because they were made early, the element that preserves them from decay being their epoch. And as a corollary to this, the history of astronomy contains (and will continue to contain) many examples of men who have been able to make advances because they were acquainted with this history and have known where to look and what to look for in the records.

Nathanael Carpenter and the "Philosophia Libera"

By GRANT McCOLLEY

To the historian of seventeenth-century cosmology, the one important English writer of the third decade was a Doctor of Divinity, Nathanael Carpenter. Among Englishmen, this now obscure divine wrote the only extended discussion of the New Astronomy to appear between 1621 and 1630.[1] In addition, his commentary was published three times within the space of four years, first in the *Philosophia Libera,* and secondly in the first book of *Geography Delineated*.[2] As recent scholars have emphasized, Carpenter's work was a vital force in completing removal of the Aristotelian incubus which so long had handicapped physical science.[3]

There remain however other significant contributions. Carpenter apparently was the first Englishman to advocate explicitly and in some detail the Fourth System of the world.[4] He also urged two premises then so essential to embryonic science, viz., that human sense, or sensory observation, is accurate and trustworthy, and that in science the inductive method is essential and valid. Intimately connected with the first was an enthusiastic acceptance of evidence provided by telescopic observation.[5]

Although historians of astronomy generally have overlooked the Fourth System of the world, this system stood for more than a half century among the most important of the "new" astronomical hypotheses.[6] Its influence was particularly strong in the decades immediately following invention of the celestial telescope. From the telescope came convincing evidence in support of the ancient theory that Mercury and Venus revolve about the sun, and that the proper planetary order is the Sun, Mercury, Venus, Earth, Mars, Jupiter, and Saturn. The observed axial rotation of Jupiter and the sun gave analogical proof of the even more ancient theory of the diurnal rotation of the central earth. Belief in the earth's axial rotation had likewise the support of a variety of arguments, both old and relatively new.

The first of these arguments was the time-honored conception that Nature does nothing the more difficult way, and therefore that she would produce night and day by a revolution of the small earth rather than by that of the immense heavens. Other arguments included the logical demonstration by Copernicus that the earth is suited for axial rotation, and the theory of Gilbert that it is a great magnet with the power necessary for such a motion. The Fourth System of the world also avoided the major scientific objection to the Copernican theory—the contention that the apparent absence of annual stellar parallax required the postulation of a universe either indefinite or infinite in extent.[7] The system likewise was considered by leading advocates as not contrary to Holy Scripture.[8]

Such were the arguments and scientific considerations which led Carpenter to reconstruct the Fourth System of the world.[9] To them he added a demonstration of a fundamental idea which became increasingly important as the century advanced. This idea was that air has weight. Since air has weight, Carpenter concluded, it will be attracted by the earth, and will move with the earth in diurnal rotation.[10] The active support of the Fourth System by a Doctor of Divinity is not unimportant. More significant however is the fact that although Carpenter's theory duplicated a relatively common hypothesis, he had constructed it eclectically.[11] This procedure definitely indicates that by the close of the first quarter of the Seventeenth Century a majority of the basic ideas of the new astronomy had entered the body of informed thought.

Equally significant are Carpenter's conclusions regarding the authority of sensory observation and the value of the inductive method. "Sensus," he wrote, "nullo modo errare potest."[12] His extended analysis ad-

mitted that on occasion things are not perceived as they are. Nevertheless, he held, the error does not proceed from the sense, as such, but from the intellect or interfering factors. As might be expected, Carpenter's absolute faith in the authority of sensory observation was closely connected with belief in the inductive method. The "scientific" judgment, he said in effect, proceeds from the collection of many "experiments," and the elicitation of conclusions drawn from their comparison and analysis.[13]

Modern though he was in many respects, Carpenter could not accept the Copernican theory. Indeed, he attacked with vigor Kepler's great discovery of elliptical planetary orbits,[14] and declared forthrightly that Aristotle was not so much opposed to reason as Copernicus was opposed to religion.[15] Yet time has proved the English divine partially correct in one objection raised against contemporary heliocentric astronomy. According to this astronomy, and in harmony with Copernicus' postulate of an unbounded and indefinite or infinite eighth sphere, the fixed stars hung motionless in the heavens. However, wrote Carpenter, by the analogy of the orbital movements of the planets, the fixed stars must have some motion.[16]

References

[1] It is the custom of historians to date as 1621 the much shorter discussion of cosmological ideas which Robert Burton included in the *Anatomy of Melancholy*, II, ii, 3 (A Digression of Air). Burton commented however upon a number of works which appeared after 1632, the date of the 4th edition. The earliest edition which could have discussed these books is the 5th, published in 1638, the year of John Wilkin's *Discovery of a World in the Moone* and of Francis Godwin's *The Man in the Moone*.

[2] *Philosophia Libera, Triplici Exercitationum Decade Proposita*, Frankfort, 1621; Oxford (? and London), 1622. *Geography Delineated Forth in Two Bookes*, Oxford, 1625. A second edition was published at Oxford in 1635. At least four editions of the *Philosophia Libera* appeared during the Seventeenth Century, the third at Oxford in 1636. All citations are to this third edition, for the use of which I am indebted to Professor George Williamson of the University of Chicago.

[3] See especially Richard F. Jones, *Ancients and Moderns*, St. Louis, 1936, pp. 68-74.

[4] According to the hypothesis which Otto von Guericke termed the "Fourth System of the World," the sun stood in the midst of the planets, and the earth in the center of the fixed stars. Mercury and Venus revolved about the sun, and the earth rotated diurnally on its axis. The planetary order was that of Copernicus and Tycho Brahe.

[5] The importance of the telescope to Carpenter is shown by his frequent references to it, *e.g.* pp. 274 ff., 344 ff. His attitude is reflected in his words, p. 345, "admirandum illud perspicillum." Among others, Jones, *op. cit.*, and Francis R. Johnson, *Astronomical Thought in Renaissance England*, Baltimore, 1937, pp. 273-275, say nothing of the influence of telescopic observations upon Carpenter's cosmological beliefs.

[6] The genesis and vogue of this system are discussed briefly in "Nicolas Reymers and the Fourth System of the World," Popular Astronomy, XLVI (1938), 25 ff. As I point out in "J. H. and the *Astronomia Crystallina*," *Annals of Science*, IV (1939), 319-321, the Fourth System was seriously urged in England as late as 1676.

[7] The interpretations of an appreciable number of astronomers who attributed to Copernicus belief in an unbounded or infinite eighth sphere are discussed in "Nicolas Copernicus and an Infinite Universe," Popular Astronomy, XLIV

(1936), 525-533; and, "The Universe of *De Revolutionibus*," *Isis*, XXX (1939), 452-472.

[8] So Reymers and J. H., as cited.

[9] *Philosophia Libera*, Oxford, 1636, pp. 272 ff., 298 ff., 344 ff., 362 ff., 385 ff.

[10] *Ibid.*, pp. 292 ff.

[11] Carpenter apparently obtained the idea (*ibid.*, pp. 362, 389) that the orbit of the sun intersected that of Mars from Tycho Brahe. The conception that Mercury and Venus move in orbital (not diurnal) revolution about the sun, described by Carpenter as a belief of the Copernicans, is attributed to "alii," unnamed.

[12] *Ibid.*, pp. 83 ff.

[13] *Ibid.*, p. 89. The fact that use of the inductive method in scientific investigation presupposes acceptance of the authority of sensory observation has largely been ignored by those historians of philosophy who begin modern empiricism either with Locke or his slightly earlier contemporaries.

[14] *Ibid.*, p. 386.

[15] *Ibid.*, p. 272.

[16] *Ibid.*, p. 385.

ARMOUR INSTITUTE OF TECHNOLOGY, CHICAGO, DECEMBER 1, 1939.

Planet Notes for April, 1940

By R. S. ZUG

NOTE: Greenwich Civil Time is employed unless otherwise stated. To obtain Eastern Standard Time subtract 5 hours, Central Standard Time, 6 hours, etc. The planetary phenomena are described as they are to be seen from latitude 45° N. The data are taken chiefly from the *American Ephemeris and Nautical Almanac*.

Sun. The sun's apparent motion will carry it from the constellation Pisces into the constellation Aries during April.

Moon. Phenomena of the moon will occur as follows:

			h	m
New Moon	April	7	20	18
First Quarter		15	13	46
Full Moon		22	4	37
Last Quarter		29	7	49
Apogee	April	5	9	
Perigee		20	19	

Eclipses. An annular eclipse of the sun will occur on April 7, 1940. The path of the annular eclipse starts in the Pacific Ocean, and passes through Mexico and the Gulf States of the United States, ending in the Atlantic Ocean. Over most of the United States the eclipse will be seen as partial. In the western portion, the partial eclipse will last from about 11:30 A.M. to 2:30 P.M., Pacific Standard Time. In the mountain states the corresponding interval is from about 1:00 P.M. to 4:00 P.M., Mountain Standard Time; in the central states, from 2:15 P.M. to 5:00 P.M., Central Standard Time; and in the eastern states, from 3:30 P.M. to 6:30 P.M., Eastern Standard Time. Representative values of the magnitude of the eclipse, or the fraction of the sun obscured at maximum eclipse, in terms of the sun's diameter, are as follows: California, 0.7; Colorado, 0.7; Minnesota, 0.6; Massachusetts, 0.6; New Mexico, 0.8; Arkansas, 0.8; North Carolina, 0.8. Further details of the eclipse are given in POPULAR ASTRONOMY, p. 21, this volume, or may be had by reference to the *American Ephemeris and Nautical Almanac* for 1940, in which in-

formation is given in detail, together with local circumstances for eighty cities in the United States.

A penumbral lunar eclipse will occur on April 22, 1940. The southern limb of the moon will approach to within three minutes of arc of the umbral shadow of the earth, so that the eclipse will be visible to the unaided eye, according to Alexander Pogo (Ref. POPULAR ASTRONOMY for January, 1940, p. 6). The eclipse will be visible from North and South America.

Mercury. Mercury will be a morning star during April. A greatest western elongation of 27° 40′ will be reached on April 12, 9^h, at which time the stellar magnitude of the planet will be +0.6.

Venus. Venus will be a brilliant evening star of magnitude —4, during April. Greatest eastern elongation of Venus from the sun will occur on April 17, 12^h, at which time the planet will be 45° 44′ from the sun. Numerical data of interest to observers are tabulated in POPULAR ASTRONOMY for January, 1940, p. 28. Venus will be in conjunction with, and 2° 11′ to the north of, the planet Mars on April 11, 0^h.

Mars. Mars will be a relatively inconspicuous evening star of stellar magnitude +1.7, during April. It will be moving eastward through the constellation Taurus, although its elongation from the sun is diminishing.

Jupiter. Jupiter will be in conjunction with the sun on April 11.

Saturn. Saturn reaches conjunction with the sun April 24.

Uranus. Uranus is moving in an easterly direction in the constellation Aries. During April, the planet will be situated about half a degree to the southeast of the 6.3 magnitude star, 53 Arietis.

Neptune. Neptune is just past opposition, and is situated in the constellation Virgo, near its western boundary. It may be found telescopically with the aid of the chart illustrating its apparent motion, which appeared in POPULAR ASTRONOMY for January, 1940, p. 31.

Occultation Predictions

(Taken from the *American Ephemeris*)

Date 1940	Star	Mag.	IMMERSION Greenwich C.T.	IMMERSION *a*	IMMERSION *b*	IMMERSION Angle E from N	EMERSION Greenwich C.T.	EMERSION *a*	EMERSION *b*	EMERSION Angle E from N
			h m	m	m	°	h m	m	m	°
OCCULTATIONS VISIBLE IN LONGITUDE +72° 30′, LATITUDE +42° 30′										
Apr. 13	115 Tau	5.3	0 46.3	—0.5	—2.1	120	1 46.4	—0.6	—0.8	251
14	292 B.Ori	6.5	0 13.5	—1.3	—1.3	99	1 26.3	—0.8	—1.6	280
15	λ Gem	3.6	0 42.5	—1.9	+0.2	64	1 38.7	—0.5	—3.0	325
15	BD+14°1850	6.4	23 36.0	—1.4	—2.2	142	0 43.1	—2.1	+0.1	254
19	p^4 Leo	5.7	2 27.5	—2.3	+0.5	74	3 23.0	—0.8	—2.6	339
20	46 B.Vir	6.5	3 34.0	—2.0	—0.2	87	4 38.9	—1.1	—2.0	325
24	73 B.Sco	6.4	7 37.4	—1.8	—0.9	117	8 50.6	—1.6	—0.6	257
24	88 B.Sco	6.4	10 24.2	—1.4	—2.6	142	11 6.1	—0.2	+0.2	222
25	29 Oph	6.4	3 38.1	..	..	36	4 7.5	..	..	344

OCCULTATIONS VISIBLE IN LONGITUDE +91° 0′, LATITUDE +40° 0′

Apr.	13	115 Tau	5.3	0	40.2	—0.8	—4.0	148	1	25.7	—1.9	+1.3	221
	13	292 B.Ori	6.5	23	47.8	—1.8	—1.7	120	1	5.2	—1.9	—0.4	256
	15	λ Gem	3.6	0	4.2	—2.1	—0.2	91	1	25.4	—1.6	—1.7	296
	15	BD+14°1850	6.4	23	20.4	..	..	174	23	49.6	..	..	215
	19	p^4 Leo	5.7	1	50.1	—1.7	0.0	107	3	5.2	—1.5	—1.1	306
	20	46 B.Vir	6.5	3	1.3	—1.6	—0.4	117	4	16.9	—1.7	—0.8	297
	24	73 B.Sco	6.4	7	7.6	—1.6	—0.8	132	8	16.9	—2.2	+0.4	250
	24	88 B.Sco	6.4	9	59.6	—1.8	—2.0	138	10	54.1	—1.1	+0.1	231

OCCULTATIONS VISIBLE IN LONGITUDE +120° 0′, LATITUDE +36° 0′

Apr.	1	16 B.Cap	6.2	12	17.5	—1.6	+3.5	27	13	8.8	—1.7	—0.7	312
	1	β Cap	3.2	12	25.2	—1.6	+3.3	29	13	20.6	—1.8	—0.6	308
	19	p^4 Leo	5.7	1	18.9	—0.7	—0.9	144	2	16.0	—1.4	+1.4	260
	20	46 B.Vir	6.5	2	38.3	—0.3	—2.4	168	3	18.8	—1.9	+2.6	241
	24	88 B.Sco	6.4	9	19.3	..	..	166	9	54.9	..	..	217
	25	166 B.Oph	6.5	12	55.3	—1.8	—0.7	90	14	12.8	—1.3	—0.9	263

The quantities in the columns *a* and *b* are given for the purpose of making these predictions useful for any place within 200 miles of the point indicated. The procedure is as follows: Subtract the longitude of the point given from the longitude of the place in question; multiply the result in degrees, taking the signs into account, by the quantity under *a* for the star to be observed; similarly, with the latitude, using *b*; apply the sum of the products, with its proper sign, to the Greenwich C.T., and obtain the predicted Greenwich Civil Time for the phenomenon at the place of observation. To obtain Eastern Standard Time it is necessary to subtract five hours; Central Standard Time, six hours, etc.

Comet Notes

By G. VAN BIESBROECK

Comet activity is reduced to a low ebb. No known comets are any longer observable at this time.

The search for PERIODIC COMET WOLF II should be continued, although the best time for observation is passing now. A continuation of the ephemeris by A. E. Levin and K. Pollock is given here with the reminder that the prediction is quite uncertain owing to large Jupiter perturbations since the previous apparition.

EPHEMERIS OF PERIODIC COMET WOLF II

		α	δ
		h m	° ′
1940 March	8	8 21.3	—14 15
	16	8 21.1	13 53
	24	8 22.8	—12 54

PERIODIC COMETS GIACOBINI-ZINNER and FAYE, due to reach perihelion on February 17 and April 23, respectively, are now behind the sun for several months and may not be seen again until late summer.

Two more known periodic comets are due to return to perihelion during this year: COMET FINLAY which has been followed at four oppositions since its discovery at the Cape Observatory in 1886, and COMET NEUJMIN which should make its first reappearance since it was found in 1929 at Simeis (Crimea). However, the first one presents itself in such unfavorable geometric conditions that there is no chance of seeing it this year. Neujmin's comet will be well situated this spring but the location is somewhat uncertain after an interval of eleven years.

Williams Bay, Wisconsin, February 12, 1940.

METEORS AND METEORITES

Meteor Notes from the American Meteor Society

By CHARLES P. OLIVIER, President

In submitting the annual report of the American Meteor Society it is gratifying to note that the total number of meteors reported from all sources is nearly eleven thousand and that the proportion of those plotted is very high indeed. Also a glance at the table will show that a large number of persons took an active part. As I mentioned some time since, when discussing our observations of the Perseids in 1939, for some undetermined reason our requests through the daily papers, last August, were badly handled. This meant that we lacked the coöperation of scores of people who, had their attention been called to it, would have reported counts which sometimes run into the thousands. Attention must also be called to the probability that even yet some reports have not been mailed and their absence lowers the total given here.

The year witnessed one regional meeting of our members, held in connection with the Milwaukee Astronomical Society. It is a pity that such meetings cannot be more frequent. I have also been of late giving serious thought as to the future of the A.M.S. As it is, having been organized and run wholly by me, its existence has been dependent upon the large financial and technical assistance furnished by the universities with which I have been connected, and upon grants made to me personally from scientific societies. Up to date, in the 29 years since its very modest beginnings when I was on the faculty of Agnes Scott College, Decatur, Georgia, to the present, literally hundreds of thousands of observations have been made by our members and associates, most of which are recorded in our files at Flower Observatory, the present headquarters. In addition, important older records have been turned over to me permanently or on indefinite loan, as for instance the work by Professor Cleveland Abbe and by Professor C. C. Trowbridge, while I had access to that of Professor W. L. Elkin long enough to prepare the essential parts of it for publication, though on his demand the records were later returned to him.

Our own files contain tens of thousands of plotted paths with accompanying record sheets fully filled in; more tens of thousands of counts made largely at shower dates, and reports on 2000 or more fireballs. These latter vary from a single report on an object to an extreme of over 400 reports on the Illinois fireball of 1929 July 25. I am also at work on as definitive a catalogue as possible of all long-enduring meteor trains. After publication, all the original reports and notes will be available for the A.M.S. A good many reprints of articles dealing mostly with meteors have been donated to the A.M.S., and a vastly larger number of reprints and books dealing with the subject belong to me personally and are available for use here. Indeed it is almost certain that the largest amount of data on meteoric astronomy, existing at any one place, is at Flower Observatory. Yet all this has been collected under my personal direction and frankly there is no available person or organization to "take over" direction should anything happen to me. It is of course my sincere hope that the A.M.S. will be a permanent and growing society, as no one would deny the services it has performed not only in America but to meteoric astronomy generally. For instance its methods of observing and recording have been, with small modifications, adopted almost universally.

The future of the A.A.V.S.O. is assured by its relations to Harvard College

Observatory, which is a very large institution with a numerous staff, and its data and records are safe there. I should like to know that the future of the A.M.S. was similarly assured, beyond any doubt, by adequate endowment so that the headquarters would permanently remain in Philadelphia, which is in many respects centrally located, in the scientific sense. These remarks are made here as I should like to have frank opinions from our active members and other friends as to how our future could best be assured. I might add that the A.M.S. is not and never has been self-supporting. The dues are not enough even to pay for the maps, blanks, and postage used annually! The various publications of the Society alone have cost to date, I estimate, about $3,000.

We take pleasure in welcoming the following persons as new members. They have joined since the last additions were published.

Astronomical Club, State Teachers College, Glassboro, New Jersey.
Wilna Connell, Box 517, Abilene, Texas.
Edwin Hazelbauer, Smithtown Ave., Bohemia, L. I., New York.
Charles E. Huckaba, 900 Meridian St., Nashville, Tennessee.
Donald V. Hutchins, Glen Elder, Mitchell County, Kansas.
Paul N. Klaas, Routh 3, Dubuque, Iowa.
Dick Knoth, 115 Mt. Vernon Ave., West View, Pennsylvania.
George E. Lambert, 3105 Decatur Ave., Bronx, New York.
James J. McKeon, Rainbow View Farm, Titusville, New Jersey.
J. N. Milnes, Kenwood, Oneida County, New York.
Edward D. Onslott, 2409 Elizabeth St., Pueblo, Colorado.
Dr. Eugene M. Parker, 1725 Wroxton Rd., Houston, Texas.
Cornelius M. Prinslow, Milwaukee, Wisconsin.
Charles D. Robinson, 234 Grand St., Newburgh, New York.
Marium Rose, 27 E. Forest Place, Rochelle Park, New Jersey.
Douglas R. Shesley, 301 23rd St., Denver, Colorado.
J. L. Sullens, 1336 Sherman St., Springfield, Missouri.

To all our members who have paid their dues for 1939 or sent in observations during the past year we are about to mail *Reprint of the Flower Observatory* No. 49. Our sincerest thanks continue to be due to the Hydrographic Office of the U. S. Navy and to the U. S. Weather Bureau, as well as to numerous individuals who are not members, for sending in reports on fireballs, interesting meteors, etc., which constantly make our files more valuable.

The following table gives the name and address of observers, with number of nights on which observations were made (not counted if less than 60 minutes of observation, as a rule), the total number of meteors recorded, and notes. As to observers, when one reported for a group, his name alone appears, and the total number of persons coöperating is given in last column. The names of (most of) the assistants have been published in former summaries of the observations, and are not repeated here. The letter C indicates a count for rates only; P that the meteors were individually plotted and fully observed; P-C that some were plotted, some counted. To do full justice to organized groups: Dr. Braun headed the Catawba College, North Carolina, group; Dr. Burch the group at State Teachers College, Radford, Virginia; Dr. Faulkner the group from Stetson University, Florida; Mr. Halbach the group of Milwaukee Astronomical Society; Dr. Kusner the group from University of Florida; Miss Shapinski reported for the Louisville Astronomical Society. The others, reporting for groups, did not represent, I believe, organizations but observers gathered together for the occasion.

Telescopic meteors were reported as follows: Walter Houston, 56; W. R. Morris, 2; Leon Laskaris, 3; G. P. Kirkpatrick, 6; D. W. Rosebrugh, 2; P. H. Taylor, 7; E. Loreta, 2. We cannot induce most A.A.V.S.O. observers to make

these simple observations, which would not take an hour of extra time per year for most of them. But we still have hopes in this direction.

In closing, may I express the hope that our regional directors, from some of whom I did not hear at all in 1939, and on whom so very much of the success of our work depends, will take their duties a little more seriously in 1940. And will our members, both those isolated as well as those fortunate enough to have others working with them, try to make the current year an even better one for the A.M.S. than 1939 was. Again, when practicable, I strongly commend coöperative work for height determination as the work which has greatest training value and which is likely to lead to most valuable results.

TABLE I

Observer and Station	Nights	Meteors	Remarks
Arslonian, Leon, San Jose, California	1	85	C
Avery, Vernon, Banks, North Dakota	40	428	P
Barton, S. G., Flower Observatory, Pennsylvania	1	16	P
Braun, Dr. M. L., Salisbury, North Carolina	3	291	C; 11 obs.
Burch, Dr. R. R., Radford, Virginia	1	196	C; 8 obs.
Coffing, Mrs. W. R., Springfield, Missouri	1	16	C; 3 obs.
Desjardins, Louis, Tulsa, Oklahoma	1	250	C; 4 obs., 8 assts.
Dietrich, W. A., Triadelphia, West Virginia	3	97	P
Dole, R. M., Cape Elizabeth, Maine	9	316	P
Faulkner, Dr. Donald, Deland, Florida	4	826	P (1938); ? obs.
Faulkner, Dr. Donald, Deland, Florida	6	440	P (1939); 8 obs.
Flowers, Denver, Belpre, Kansas	2	33	C; 2 obs.
Furtmann, Mrs. A., Milwaukee, Wisconsin	3	101	C; ? obs.
Gorson, Robert, Philadelphia, Pennsylvania	1	24	C
Green, Gordon, Pittsfield, Massachusetts	4	117	P-C
Habereck, H. J., Bayonne, New Jersey	2	44	C
Halbach, E. A., Milwaukee, Wisconsin	3	146	P; 8 obs.
Hardy, C. T., Fremont, Ohio	9	520	C; 4 obs.
Heaton, Gordon, Sheffield, Alabama	7	171	P; 2 obs.
Hoag, A. A., Barrington, Rhode Island	3	84	C
Huckaba, C. E., Nashville, Tennessee	3	113	P
Jewell, Miss, Grandview, Tennessee	4	75	C
Johnson, H. M., Des Moines, Iowa	11	488	P; 7 obs.
Jones, Miss Margery, Pomona, California	1	68	C
Khan, Prof. M. A. D., Begumpet, India	39	424	P-C
Klaas, Faul, Dubuque, Iowa	16	338	P
Komski, Koziro, Kamaya, Japan	9	168	P
Kusner, Dr. J. H., Gainesville, Florida	3	331	P; 15 obs.
Lambert, G. E., Bronx, New York	27	170	P
Larrabee, Miss L. M., Honolulu, T. H.	1	54	C
Leerman, Joseph, Baltimore, Maryland	12	84	C
Loreta, Eppe, Bologna, Italy	43	1131	P-C
McArthur, Campbell, N. Quincy, Massachusetts	5	138	P
Mastell, Robert, Hibbing, Minnesota	7	141	P
Metz, Louis, Wheeling, West Virginia	1	326	C; 4 obs.
Morris, W. R., Chicago, Illinois	3	30	P
Neale, J. J., New Haven, Connecticut	6	306	P; 6 obs.
N. Y. Junior Astronomical Club, New York City	2	156	C-P; ? obs.
Olivier, C. P., Flower Observatory, Pennsylvania	1	32	P
Parker, P. O., Ooltewah, Tennessee	3	21	P
Partridge, Daniel, Pawtucket, Rhode Island	2	260	C
Pirsig, Gerald, Blue Earth, Minnesota	5	39	P
Pruett, Prof. J. H., Eugene, Oregon	1	91	C
Radamacher, Mrs. E. S., Hamden, Connecticut	6	168	P-C; 4 obs.
Robinson, C. D., Newburgh, New York	2	11	P
Shapinski, Miss M., Louisville, Kentucky	4	312	C-P; 8 obs.
Smith, F. W., Thiells, North Carolina	4	14	P?
Stone, W. R., Santa Barbara, California	15	165	P

Observer and Station	Nights	Meteors	Remarks
Sullens, J. L., Springfield, Missouri	2	197	P
Thomson, Mrs. F., Eugene, Oregon	1	27	C
Thomson, Miss B. J., Eugene, Oregon	1	31	C
Wallace, William, Cleveland, Ohio	9	130	P?
Watson, P. S., Baltimore, Maryland	2	55	P; 2 obs.
Wetherill, George, Upper Darby, Pennsylvania	2	92	P
Wood, Fergus, Eugene, Oregon	1	36	C
Total		10,423	
Casual meteors		53	
Fireballs		180	
Telescopic		78	
Photographed		6	
Grand Total		10,742	

Flower Observatory, Upper Darby Pennsylvania, 1940 February 17.

Schiaparelli's "Shooting Stars"*

By C. C. WYLIE

Schiaparelli's "Stelle Cadenti" was published in Florence, Italy, in 1867. It opens with an introductory discussion of the older theories of meteors, and then gives as proof of a connection between comets and meteors, the identical orbits for, first, the Leonids and Temple's Comet of 1866; second, the Perseids and Tuttle's Comet of 1862; third, the Andromedes and Biela's Comet; and fourth, the Lyrids and the comet of 1861.

The author gives a rather complete discussion of the diurnal and annual variation of meteors in number and direction. He tells us that the annual variation was noticed by Brandes in 1823, and that in 1827 Brandes published the meteoric proof of the revolution of the earth about the sun. It is interesting that this was the second proof of the revolution of the earth published, only the aberration of light preceding it.

The author refers to the more complete studies on the number and direction of motion of meteors made by Herrick and Herschel, and also to the early work on radiants by Heiss, Gregg, and others. He notes that the radiants show a concentration on the ecliptic similar to the concentration of comet orbits on the ecliptic. From a study of these variations and radiants, the author quotes Newton as concluding that the velocity of the average meteor must be a little under that of the average comet; but his own conclusion is that the average velocity is a little higher, almost exactly equal to the parabolic.

The author discusses the effect of the motion of the earth in its orbit on the velocity of the meteor and derives the formula for the effect of the attraction of the earth on the velocity. Including both effects, he gives the maximum velocity for a parabolic meteor coming from near the apex and from near the anti-apex.

The author derives from data the diurnal and annual variation in frequency for meteorites, and notes that it is almost opposite to the variation of frequency of shooting stars. He explains this as due to the fact that low velocity meteors have

*Read by title at the Seventh Annual Meeting of the Society for Research on Meteorites, Columbus, Ohio, 1939, December 28-30. This paper was mailed to Professor La Paz of Ohio State University with the request that he present it at the meeting. It was delivered by mistake to another person who held it until school reopened after the holidays, hence it was read by title only.

a better chance of surviving until they reach the surface of the earth.

The author gives the formulas for calculating the effect of the earth's attraction on the number of meteors, and also for zenith attraction and diurnal aberration. He discusses the perturbations of meteoric streams by planets and refers to Leverrier's well-known calculation that the Leonids were brought from a practically parabolic orbit to their present orbit by an approach to Uranus in 126 A.D. From calculations which he makes himself, Schiaparelli concludes that Leverrier's result has no significance, and that if the Leonids were brought into their present orbit by a planet, it is much more probable that Jupiter or Saturn was responsible.

The author devotes considerable space to a discussion of the possible origin of comets and meteors. He takes it for granted that most meteors and most comets have come into the solar system on hyperbolic rather than elliptical orbits. He assumes that the elliptical orbits for both comets and meteors are the result of perturbations by planets, including the earth; also that, after each perturbation, the comet or the group of meteors is more dispersed. He assumes that nebulous material in interstellar space is the source of both comets and shooting stars.

The author discusses the zodiacal light and the gegenschein, mentioning especially observations of 1730 and 1803, Brorsen's announcement of the gegenschein in 1855, and the classical work of Jones on the zodiacal light. He doubts, however, that the zodiacal light and gegenschein can be explained as due to meteoric material.

The author refers to the fact that the general public often calls a meteor a "comet" and explains this as due to the resemblance between a persistent meteor train and a comet. He assumes that in the old stories of a comet dropping stones to the earth, the "comet" was the train of the meteor which dropped the stones.

The book has considerable historic value. For example, the meteoric proof of the earth's revolution is not given in textbooks and is not well known. Although published first in 1827, it has been announced twice in recent years in the "Meteors and Meteorites" section of POPULAR ASTRONOMY, first in 1930, and again, in less complete form, in 1938. For meteors, the data on diurnal and annual variation in frequency and direction are quite complete. For meteorites, the data on diurnal and annual variation in frequency are good. The formulas Schiaparelli gives for the effect of the earth's attraction and of diurnal aberration on the velocity and direction of fall of a meteor are used regularly now in accurate work on the paths and orbits of fireballs.

This book was translated into German in 1871 by Boguslawski. At present, however, both the original Italian "Stelle Cadenti," and the German translation, "Sternschnuppen," are hard to get, and when one is obtained, probably it will be in too poor condition for regular use.

Because of the importance of the book, and the scarcity of copies, we have made an English translation of "Stelle Cadenti." Much, but not all, of this English translation has been compared with the German translation by Boguslawski, using a copy lent us by Dr. S. A. Mitchell, Director of the Leander McCormick Observatory.

From first reports we have assumed that there is a sufficient demand for this to justify our smoothing out the somewhat literal English, and printing the translation, probably as a *Contribution of the University of Iowa Observatory*. Expressions of opinion by members of the Society will be welcomed.

University of Iowa, December 15, 1939.

Contributions of the Society for Research on Meteorites

Edited by FREDERICK C. LEONARD,
Department of Astronomy, University of California, Los Angeles

President of the Society: H. H. NININGER, Colorado Museum of Natural History and American Meteorite Laboratory, Denver
Secretary of the Society: ROBERT W. WEBB, Department of Geology, University of California, Los Angeles

Report of the Seventh Annual Meeting

By LINCOLN LA PAZ, *Secretary pro tempore*

The Seventh Annual Meeting of the Society for Research on Meteorites was held in conjunction with the One Hundred-Fifth Meeting of the American Association for the Advancement of Science on December 28, 29, and 30, 1939, at the Ohio State University in Columbus, Ohio. The following eight members were in attendance at one or more of the sessions of the Society, which were open also to and were well attended by other scientists (non-members) and the general public: Hugh G. Harp (Springfield, Ohio), Miss L. J. La Paz and Lincoln La Paz (Columbus, Ohio), Frederick C. Leonard (Los Angeles, Calif.), H. A. Meyerhoff (Northampton, Mass.), G. Edward Pendray (New York, N. Y.), W. Carl Rufus (Ann Arbor, Mich.), and E. S. C. Smith (Schenectady, N. Y.).

The number of papers read at the Columbus meeting was twenty-one. Eleven of these papers were presented personally by their authors. On the morning of December 29, a joint symposium was held with Section E (Geology and Geography) of the Association, which was participated in by several authorities in the fields of geology and meteoritics. Five papers were read at this symposium, four by their authors. On the morning of December 30, a joint symposium was held with Section D (Astronomy) of the Association, at which leaders in the fields of meteoric astronomy and meteoritics presented five papers—four read by their authors and one by title only, in the absence of the author and the manuscript. These symposia were the outstanding features of the sessions of the Society held on the campus of the Ohio State University.

The exhibit of the Society in booth No. 23 at the Association Science Exhibition in the Columbus Auditorium was constantly the center of a crowd of interested and inquisitive onlookers. A model meteorite-detector, which the visitor could operate for himself, was probably the most interesting single feature of the exhibit, but much attention was directed also to the meteorites recently found by instrumental search, the "Missing-Meteorite Bureau," and the collection of tektites (lent by Dr. F. C. Leonard), of polished and etched sections of meteorites (lent by Dr. H. H. Nininger), of priceless Amerind artifacts of meteoritic iron (lent by the Ohio State Museum), of pseudo-meteorites, of rocks metamorphosed by meteoritic impact, and of literature relating to meteoritics. The sales of publications, issued and sponsored by the Society, were larger than at any previous meeting.

No meeting of the Council was held, since a quorum of its members was not present. In the absence of the Secretary (Dr. R. W. Webb), Dr. Lincoln La Paz, on appointment by the Executive Committee of the Council, served as Secretary *pro tempore*.

[The Secretary has to report that the draft of the *Constitution* and the *By-*

Laws of the Society, as proposed by the Council for adoption at, and circulated with the call for, this meeting, was duly adopted by unanimous vote of some 54 members, cast largely by mail. These new instruments became effective upon the adjournment of the Seventh Annual Meeting.—ROBERT W. WEBB, *Secretary*.]

The program of the meeting and the list of papers read at its various sessions follow:

PROGRAM OF THE SEVENTH ANNUAL MEETING

Ex-President Frederick C. Leonard presiding

Thursday, December 28

2:00 P.M., Room 205, Mendenhall Laboratory, Ohio State University: Session for papers, featured by Ex-President Leonard's report on the discovery, identification, and recovery of the great Goose Lake, California, siderite.

Friday, December 29

9:30 A.M., Room 205, Mendenhall Laboratory, Ohio State University: Joint session with Section E (Geology and Geography) of the American Association for the Advancement of Science, for presentation of papers dealing with "Meteorite Falls and Meteoritic Accretion." The importance of the development of instrumental equipment permitting a systematic search for meteorites in three dimensions, rather than the usual haphazard search, restricted to the surface of the earth, was well brought out by Dr. Leonard in the discussion following Dr. La Paz's paper. The simple argument by which Dr. F. G. Watson, Jr., obtained an upper bound on the rate of meteoritic accretion and the elaborate study of the metallurgical features of some Odessa, Texas, and Canyon Diablo, Arizona, meteorites, reported on by Dr. J. O. Lord, merited the approval accorded to them.

2:00 P.M., Room 205, Mendenhall Laboratory, Ohio State University: Session for papers. Mr. J. Foster's program for accurate determination of meteoritic densities and the sequence of papers by Professors H. O. Beyer and W. C. Rufus on the tektite problem aroused much interest and discussion.

Saturday, December 30

9:00 A.M., Room 154, Chemistry Building, Ohio State University: Joint session with Section D (Astronomy) of the American Association for the Advancement of Science, for the consideration of papers on meteor spectra and allied topics. At this meeting, Dr. P. M. Millman gave a most interesting report on his fundamental researches on the spectra of meteors. Vigorous discussion followed the presentation of each paper read at this stimulating joint session.

LIST OF PAPERS READ AT THE SEVENTH ANNUAL MEETING

[The papers are numbered in the order in which they were read.]

AFTERNOON SESSION, DECEMBER 28

[1]*1. The Finding, Identification, and Recovery of the Goose Lake, California, Siderite. FREDERICK C. LEONARD, University of California, Los Angeles.

[2]2. The Tamentit, Tuat, Morocco, Meteorite. F. W. CASSIRER, 5 Rue Raffaelli, Paris, 16, *France*. (Read by F. C. Leonard.)

[3]3. Old and New Localities of Moravian and Bohemian Moldavites. JAN OSWALD, Českí-Budějovice, *Bohemia*. (Translated and communicated by F. W. Cassirer, 5 Rue Raffaelli, Paris, 16, *France*, and read by F. C. Leonard.) [During the reading of these first three papers by the Ex-President, the chair was occupied by the Secretary *pro tempore*.]

MORNING JOINT SESSION, DECEMBER 29, WITH SECTION E (GEOLOGY AND GEOGRAPHY) OF THE A.A.A.S., ON "METEORITE FALLS AND METEORITIC ACCRETION"

[4]*4. The Benld, Illinois, Meteorite of September 29, 1938. BEN HUR WILSON, Joliet Junior College, Joliet, Illinois.

5. The Possibility of an Additional Meteorite Crater near Odessa, Texas. JOHN D. BOON and CLAUDE C. ALBRITTON, JR., Southern Methodist University, Dallas, Texas. (Read by Kirk Bryan, Harvard University, Cambridge, Mass., Vice-President, A.A.A.S., and Chairman, Section E.)

*6. A Preliminary Report on an Instrumental Search for Meteorites. LINCOLN LA PAZ, Ohio State University, Columbus.

*7. The Rate of Meteoritic Accretion. FLETCHER G. WATSON, JR., Harvard College Observatory, Cambridge, Mass.

*8. The Structural History of Iron Meteorites. J. O. LORD, Ohio State University, Columbus.

AFTERNOON SESSION, DECEMBER 29

[5]9. The Portland, Oregon, Meteor and Recovered Meteorite [Washougal, Washington]. J. HUGH PRUETT, University of Oregon, Eugene. (Read by L. La Paz.)

*10. On the Determination of Meteoritic Densities. JOSEPH FOSTER, Observatory, State University of Iowa, Iowa City.

[6]11. An Unexplained Craterlet near Chickasha, Oklahoma. OSCAR E. MONNIG and ROBERT G. BROWN, 312 W. Leuda St., Fort Worth, Texas. (Read by L. La Paz.)

[7]12. A Meteor Train Photographed from an Airplane. OSCAR E. MONNIG, 312 W. Leuda St., Fort Worth, Texas. (Read by L. La Paz.)

13. Experiments with a Rotating Meteor-Spectrograph. WALTER T. WHITNEY, Frank P. Brackett Observatory, Pomona College, Claremont, Calif. (Read by L. La Paz.)

14. Ten Years with the Leonids. WALTER T. WHITNEY. (Read by L. La Paz.)

[8]15. Philippine Tektites and the Tektite Problem in General. H. OTLEY BEYER, University of the Philippines, Manila, *Philippine Islands*. (Read by W. C. Rufus.)

[9]*16. An Astronomical Theory of Tektites. W. CARL RUFUS, Observatory, University of Michigan, Ann Arbor.

MORNING JOINT SESSION, DECEMBER 30, WITH SECTION D (ASTRONOMY) OF THE A.A.A.S., ON "METEOR SPECTRA AND OTHER SUBJECTS OF ASTRONOMICAL AND METEORITICAL INTEREST"

*17. Instrumental Equipment for Photographing Meteor Spectra. P. M. MILLMAN, David Dunlap Observatory, University of Toronto, Richmond Hill, Ont., *Canada*.

[10]18. An English Translation of Schiaparelli's *Shooting-Stars*. C. C. WYLIE, Observatory, State University of Iowa, Iowa City. (Read by title, since the manuscript was not received until after the adjournment of the meeting.)

*19. A Survey of Meteor Spectra. P. M. MILLMAN.

*20. The Spatial Temperatures of Meteorites. FLETCHER G. WATSON, JR.

*21. The Distribution of Ray Craters on the Moon. LINCOLN LA PAZ.

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*Papers starred were presented personally by their authors.

References

[1] Published in full, with 6 figs., under the title of "The Goose Lake Siderite: The Largest Known Meteorite of California," in *The Griffith Observer* (Griffith Observatory, Los Angeles, Calif.), **4**, No. 1, 1-8, Jan., 1940.

[2] Published in the *Pub. Astr. Soc. Pacific*, **52**, No. 305, Feb., 1940.

[3] Published originally in a 20-p. pamphlet (with 2 figs.) in the Czech language, bearing the following inscription on the title page: "Stará a Nová Naleziště Vltavínů Moravských a Českých. Zvláštní otisk z Časopisu Národního musea 1936. Tiskem Pražské akciové tiskárny.—Nákladem vlastním." The English translation of a part of this, supplied by M. Cassirer and read at the meeting, may be published in the near future under the auspices of the Society.

[4] Several accounts of this aërolite have already been published; see, *e.g.*, *P. A.*, **46**, 548-58, 6 figs., 1938.

[5] See *C.S.R.M.*, *P. A.*, **47**, 500-4, 2 figs., 1939, for two accounts of the Washougal, Washington, aërolite.

[6] Abstract in *C.S.R.M.*, *P. A.*, **48**, 94, Feb., 1940.

[7] Abstract, with 2 figs., in *C.S.R.M.*, *P. A.*, **48**, 93, Feb., 1940.

[8] Published in *C.S.R.M.*, *P. A.*, **48**, 43-8, Jan., 1940.

[9] Published in *C.S.R.M.*, *P. A.*, **48**, 49-51, Jan., and 92-3, Feb., 1940.

[10] Published in *P. A.*, **48**, 152-3, March, 1940.

The Distribution of the Recognized Meteorites of North America*

By Lincoln La Paz,

Department of Mathematics, Ohio State University, Columbus

Introduction

Several writers have already called attention to a lack of *uniformity* in the horizontal distribution of the recognized meteorites[1] of North America. According to Farrington,[2] the apparent *scarcity* of meteorites in certain localities, *e.g.*, British North America and the western United States, is due solely to the widely dispersed population of these areas. There are instances where this explanation obviously breaks down, as, *e.g.*, in the case of the State of Illinois, an old, well populated area of 56,000 square miles containing within its boundaries at most 2 recognized meteorites (Tilden, Benld), both witnessed falls. In such cases, inspection of a map showing the extent of glaciation in North America will suggest usually that most of the meteorites of the area in question are either buried under glacial drift or effectively concealed amid innumerable surface boulders and erratics of ice-age origin. Fisher[3] has already suggested this *glacial masking* as the chief cause of the relative abundance of irons south of the Ohio River in comparison with the sparse siderite population north of this stream. The opposite phenomenon of *meteoritic concentration* is not only more impressive, but the explanations of its causes which have been proposed are less satisfactory than those offered to account for the existence of regions nearly or quite destitute of meteorites. Thus, to quote Farrington:[4]

> "The greatest massing of meteorites in the whole province of North America occurs in the region of the southern Appalachians, where the States of Kentucky, Virginia, Tennessee, North Carolina, Georgia, and Alabama adjoin. A circle with a radius of 300 miles drawn about Mt. Mitchell, North Carolina, as a center, will include nearly half of the known meteorites of North America. Twenty-five of these, or nearly half of the known falls of the continent, are observed falls, and it would seem possible at first thought that many of the meteorites in this area might have come from a single shower. This would reduce the number, but the writer has made a careful study of the history of each meteorite and its geographic relation to those of similar character without finding any support for such a view. Not only does the area contain a large number of ob-

*Read at the Sixth Annual Meeting, Richmond, Virginia, December, 1938. Abstract published in *C.S.R.M.*: *P. A.*, **47**, 276-7; **2**, No. 2, 111, 1939.

served falls, but the finds embrace a variety of types larger than any known to be produced by a single shower. Meteorites of a single type are, as a rule, much more widely scattered than those of any single observed shower. As regards population in the area, conditions are only moderately favorable, since the area is not very thickly settled. The climate of the region is moist, the average yearly rainfall being 50-60 inches, so that a relatively rapid disintegration of iron meteorites might be expected. Yet in spite of so many conditions unfavorable to their occurrence in large numbers, meteorites are superabundant in this area. This seems to leave little doubt that some force tends to bring about their concentration here. It is noteworthy that this region includes the highest summits of the Appalachians, and this suggests either the presence of an extra-gravitational force or that a purely obstructive effect has been exerted by the high peaks. Studies of the gravitational effects of mountain masses indicate no force seemingly sufficient to affect the fall of a meteorite, though some such force may exist. Magnetic influences may also be suggested."

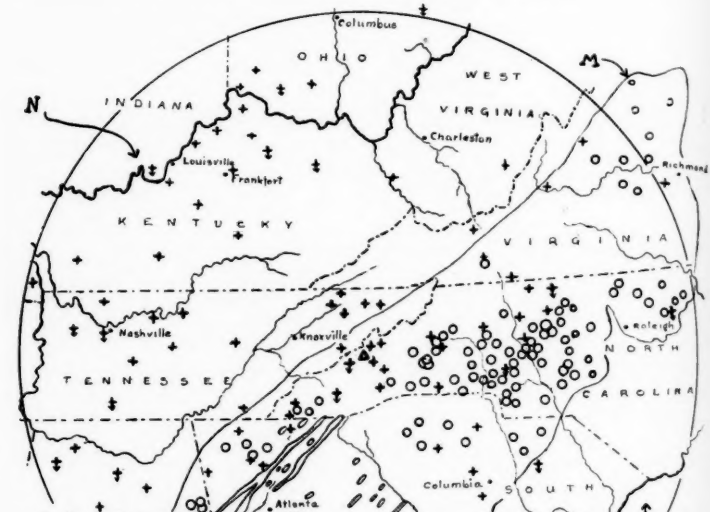

FIGURE 1

RECOGNIZED METEORITES AND PLACER FIELDS IN THE FARRINGTON CIRCLE, *F*

Δ = Mt. Mitchell, North Carolina.

‡ = witnessed fall; + = find (after Farrington).

○ = intensively placered area (after Jackson and Knaebel).

The contour *M* is the approximate boundary of the placered region (chiefly after Nitze and Wilkens).

We shall call the circle described by Farrington in the foregoing quotation the "Farrington circle" and denote it by F (see Fig. 1). In the same *Memoir*, Farrington stresses also the large number of meteorites discovered in the State of Kansas and the concentration of irons along the Cordilleras in Mexico. From his remarks it is apparent that he regarded a *uniform* distribution as the only one consistent with the assumption that the places of find of meteorites constitute a random set of points. The observed non-uniformity of distribution of recognized meteorites therefore led him, curiously enough, to conclude that meteorites do not *fall* at random over the North American continent and hence that this area must be the seat of forces, gravitational or otherwise, acting to produce a non-random infall. Olivier,[5] commenting on the remarks of Farrington, writes that "While such a distribution of falls most probably is due to mere chance, yet it is remarkable enough to be worth mentioning." Fisher, in a paper already cited,[3] attributes the concentration of recognized meteorites in uplands and in mountains to the general hardness of the ground in such regions but admits that such an explanation breaks down in Canada and New England where, also, the surface is "largely hard" but known meteorites are few. Very recently the suggestion has been made that the high percentage of North American irons discovered in the southern Appalachian region is the result of a wide-spread meteoritic shower accompanying the collision of the earth and the comet which is conjectured to have produced the Carolina "Bays."[6]

It is the purpose of the present paper to make a critical examination into the need for and the validity of the explanations which have been advanced to account for the observed lack of uniformity in the horizontal distribution of recognized North American meteorites. On the one hand, it is found that the forces conjectured by Farrington to give rise to a non-random infall of meteorites are not competent to produce such an effect. On the other hand, it turns out to be exceedingly unlikely that the most important meteoritic concentration in North America, that in the Mt. Michell area, is the result of "mere chance." Arguments are adduced to show that the real cause of this concentration is the vast amount of small-scale placer-mining which has been performed in and near the southern Appalachian gold-field, the oldest placer-ground in the United States. It is probable that the same explanation will account for the concentration of irons along the Mexican Cordilleras, the oldest gold-mining country on the North American continent. On the contrary, the existence of such a minor meteoritic concentration as occurs in Kansas is found to be quite compatible with the hypothesis that the places of find of the recognized meteorites of the Great Plains region constitute a random set.

§1. Comparison of Theoretical and Observed Distributions

In this section we shall limit attention to the distribution of those meteorites recognized by Farrington up to 1909 in British North America, the United States north of the parallel of $+29°$, and the extreme northern part of Mexico.[7] These meteorites are scattered over a region R whose boundaries are, roughly, the parallels of $+29°$ and $+53°$ and the Atlantic and Pacific coast lines of the North American continent.[8]

As we shall verify in the next section, Farrington's belief in the existence of influences of terrestrial origin producing a *non-random infall* of meteorites upon R appears to be without foundation. We shall therefore suppose that the *meteoritic infall* over R is *random*. If, now, R were a region of unchanging topography and soil mantle, covered with a uniform growth of vegetation and supporting a uni-

formly distributed population of individuals each equally likely to discover and report meteorites, if the annual precipitation were the same throughout R, and if various other essential conditions were fulfilled, then the set of recognized meteorites might legitimately be regarded as determining a set of points (places of find) distributed at random in R. Although Farrington and others appear to have believed that such a set would consist necessarily of points distributed *uniformly* in R, it is quite unlikely that such would be the case. It is easy to see why this fact is so by examination of the following simple, analogous situation: Let n, h, be integers greater than unity. Suppose that we effect an equi-areal partition of R into n subregions and that $N = h \times n$ points are then taken at random in R.[9] The possible resultant distributions range from that in which all N points fall into a single subregion to that (denote it by u) in which each subregion contains exactly h points. Denote by S the set whose elements are these very numerous possible resultant distributions. As is well known, the element of S with the maximum probability of occurrence is u, but it is highly improbable that precisely the specific (uniform) distribution, u, will occur in any particular case, for all the other elements of S (each giving a non-uniform distribution of the N points), while less probable *individually*, are much more probable in *sum* than u. The probable distribution arising from a random selection of N points (places of find) in R is therefore a *non-uniform* one, and it is evident that this conclusion holds whether N is a multiple of n or not.

If we wish more precise information as to the nature of this probable distribution, we may have recourse to a familiar theorem of Bernoulli[10] which asserts that, if N points are taken at random in R, then the probability, $q(j)$, for a particular one of the n equal areas into which R is subdivided to contain exactly j points ($j = 0, 1, 2, \ldots, N$) is given by the expression

$$q(j) = N!\, p^j (1-p)^{N-j} / j!(N-j)!, \tag{1}$$

where $p = 1/n$. From (1), the *probable number* of subregions of R, each containing exactly j points, is seen to be

$$\nu(j) = n \times q(j) = n \times e^{-\mu} \times \mu^j / j!, \tag{2}$$

where $\mu = pN$. The second expression $\nu(j)$ in (2) is derived by use of the Poisson approximation for the right member of (1).[11]

In order to give some indication of the accuracy with which formula (2) predicts the values of $\nu(j)$ for random distributions of points in a region, we shall apply it to the following ideal case: Suppose s is a plane square of edge ten. Subdivide s into $n = 100$ small equal squares by lines which partition the original square into ten columns (numbered 0 to 9) and ten rows (also numbered 0 to 9). Distribute $N = 250$ points at random over s by use of the following procedure: For each point, P, let a mantissa be chosen at random from a seven-place table of logarithms; if the penultimate digit in this mantissa is c ($0 \leq c \leq 9$) and the ultimate digit is r ($0 \leq r \leq 9$), then P is placed in the c-th column and the r-th row in s. A comparison between the actual values of $\nu(j)$ observed in two experi-

TABLE 1

	j	0	1	2	3	4	5	6	7	$\geqslant 8$
(1)	$\nu_o(j)$	7.	23.	28.	17.	13.	7.	4.	1.	0
	$\nu_c(j)$	8.1	20.5	25.7	21.5	13.4	6.7	2.8	1.	0.4
(2)	$\nu_o(j)$	10.	18.	27.	18.	16.	8.	1.	2.	0
	$\bar{\nu}_o(j)$	8.5	20.5	27.5	17.5	14.5	7.5	2.5	1.5	0

mental distributions of the sort just described and the values of $\nu(j)$ computed from (2), with $n = 100$ and $N = 250$, is given in Table I. The first and third rows in the body of this table contain the values $\nu_o(j)$ observed in the first and second experiment, respectively; the second row, the computed values $\nu_c(j)$; and the fourth row the average of the observed values of the $\nu_o(j)$.

In each case, the agreement between the computed values $\nu_c(j)$ and the observed values, $\nu_o(j)$, is satisfactory in the sense that, while perfect concordance does not exist (and of course was not to be expected), still there does not appear to be any conspicuous disagreement between the two series of numbers. It is of course possible to give a precise measure of the degree of concordance between the two sets of numbers, *i.e.*, to indicate the probability that $|\nu_o(j)/n - q(j)| < \epsilon$, where ϵ is an assigned positive number, but for our purposes, here and hereafter, such a penetrating mathematical discussion is unnecessary.

The evidence of Table 1, as far as it goes, suggests that in an ideal case, where the hypothesis of Bernoulli that the N points are distributed at random over the region in question is fulfilled, there will be no systematic difference between the computed and the observed values of $\nu(j)$.[12] The validity of this conclusion is one of the best attested facts in the field of probability. However, when formula (2) is employed on distributions where Bernoulli's hypothesis is only approximately fulfilled, systematic differences between $\nu_c(j)$ and $\nu_o(j)$ are to be anticipated. In such applications of formula (2) to distributions of the places of find of meteorites (meteoritic distributions) as we shall make in the sequel, it is possible to forecast the general character of the systematic differences to be expected. Thus, the hypothesis of Bernoulli fails to be exactly satisfied for such meteoritic distributions, because many of the subregions in the area under consideration have been relatively inadequately searched for meteorites, while a few, in which the farming population is denser and more intelligent or where rock-free soil or placer-mining increases the chances for the discovery of meteorites, have been relatively well searched. These departures from the ideal situation to which Bernoulli's formula (2) actually applies will tend to give evidently too many subregions with no recognized meteorites in them, too few with the "average" number (N/n) of meteorites in them, and again too many with large numbers of meteorites in them. This forecast is strikingly confirmed by the Tables of $\nu_c(j)$- and $\nu_o(j)$-values for meteoritic distributions, which will be given later.

In order to apply (2) to such meteoritic distributions in R, we must exhibit an equi-areal partition of this region. The partition of R effected by the meridians and parallels of latitude does not have the equi-areal property. However, if we denote by B the area of the subregion of R bounded by the parallels of $+40°$ and $+44°$ and a pair of meridians (m_1, m_2) differing in longitude by, say, 4°, then it is easy to specify a partition of R for which each subregion is of area B. We need note only that, by the proper spacing of parallels of latitude above $+44°$ and below $+40°$, we can effect a subdivision of the area lying between m_1 and m_2 into subregions (which for convenience may be called "squares"), each of area B. To prove that this condition is so, note that if A_{pq} is the area of that "square" lying between latitudes ϕ_p and ϕ_q and A_{rs} the area of that "square" lying between latitudes ϕ_r and ϕ_s, $\phi_p < \phi_q \leqq \phi_r < \phi_s$; then

$$A_{pq}/A_{rs} = (\sin \phi_p - \sin \phi_q)/(\sin \phi_r - \sin \phi_s). \tag{3}$$

Hence, $A_{pq} = A_{rs}$, if only

$$\sin \phi_p - \sin \phi_q = \sin \phi_r - \sin \phi_s \ ; \tag{4}$$

but, when any 3 of the 4 latitudes ϕ_p, ϕ_q, ϕ_r, and ϕ_s are assigned, it is possible to find always a value of the remaining quantity which satisfies (4).

By use of (4), it can be shown that the parallels of latitude, +29° 9′, +32° 37′, +36° 13′, +40°, +44°, +48° 17′, and +52° 59′, taken in consecutive pairs, intercept regions of area B lying between consecutive pairs of the set of meridians W. 61°, W. 65°, W. 69°, . . . W. 121°, and W. 125°. Examination of the spherical "checkerboard" generated in this manner will show that many of its "squares" fall over the Atlantic Ocean, the Pacific Ocean, or the Great Lakes. Since the probability of a meteorite discovery in a water area is practically zero,[13] such "squares" must be deleted. For the same reason, a row of "squares" extending over an unpopu-

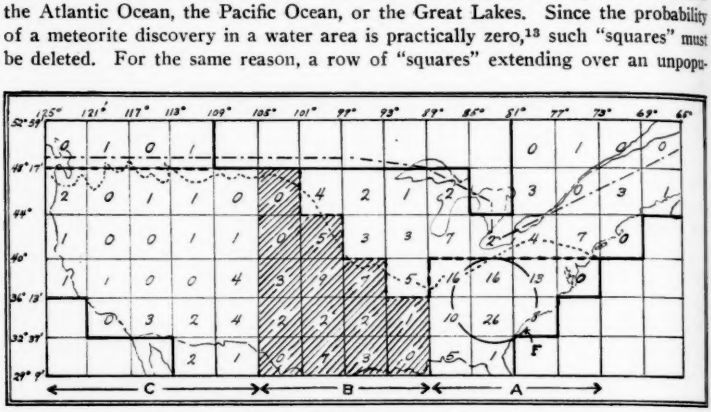

FIGURE 2

DISTRIBUTION OF RECOGNIZED METEORITES IN THE REGION R

R = totality of small "squares" inclosing numbers.
F = "Farrington Circle."
A = Southern Appalachian Province.
B = Great Plains Province.
C = Western Province.
The dotted line is the southern limit of the ice at the time of maximum glaciation (according to Salisbury).
The dashed line is the *adopted* boundary between the glaciated region G and the unglaciated region U.

lated strip of muskeg land in central Canada must be omitted. After such deletions, we are left with the region shown in Fig. 2, which exhibits the precise boundaries of that region R to which we have referred constantly and which was first roughly delimited earlier in this section. The small circle F in this figure is the "Farrington circle" with center at Mt. Mitchell and radius equal to 300 miles. It is found that R contains $n = 68$ "squares," each of area $B = 58{,}550$ square miles. The integer appearing in each of these various "squares" indicates the number of meteorites recognized by Farrington, for which the places of fall lie in the square. The sum of these integers is $N = 211$. If, now, (2) were employed with these values of n and N, the resulting quantities $\nu(j)$ would characterize the probable distribution of 211 meteorites in R under the hypothesis, H: *that each of the 68 "squares" in R is equally likely to have been that in which a particular one of the meteorites was found.* A moment's reflection on the great disparity of the "squares" as regards those conditions under which meteorites are preserved, discovered, and reported will bring one to the conviction that the assumption H is not even approximately fulfilled in R as a whole. Calculation serves only forcibly to substantiate this con-

clusion; *e.g.*, it is found that the observed number of "squares" containing 10 or more recognized meteorites is 130 times the calculated number of such "squares."[14] We are therefore next faced with the problem of dividing R into 2 or more subregions within each of which H is (approximately) fulfilled.

The existence of glaciated and unglaciated territory in R suggests at once a natural subdivision of this area into 2 parts, the dividing line between which is represented rather closely almost everywhere by the dashed line in Fig. 2. We shall denote by G the subregion of R north of this line and by U the subregion south of it. It is found that $n_G = 24$, $N_G = 50$, while $n_U = 44$ and $N_U = 161$. We now employ (2), with n replaced by n_G and N replaced by N_G, to compute to the nearest tenth the values of $\nu(j)$. The results are exhibited in the first row in the body of the following table. The observed values of $\nu(j)$ for G appear in the second row of this table.

TABLE 2

j	0	1	2	3	4	5	$\geqslant 6$
$\nu_c(j)$	3.0	6.2	6.5	4.5	2.3	1.0	0.5
$\nu_o(j)$	7.	6.	2.	4.	2.	1.	2.

The agreement is very fair in view of the diversity of conditions affecting discovery, existing in the wide longitude-range covered by G. Such discrepancies as appear between $\nu_c(j)$ and $\nu_o(j)$ are of precisely the sort anticipated in earlier remarks. We seem justified, therefore, in regarding the distribution of recognized meteorites in G as essentially random in nature. On the contrary, when we employ (2) with $n = n_U$ and $N = N_U$, we find values of $\nu(j)$ differing very greatly from the values of this quantity actually observed in U; *e.g.*, there are 50 times as many observed "squares" containing 13 or more meteorites as is predicted by (2). In the case of U it is therefore quite unlikely that the observed distribution is the result of mere chance.

The chief discrepancy between the computed and observed values of $\nu(j)$, for U as for R, owes its origin to the southern Appalachian meteoritic concentration, *i.e.*, the large number of meteorites found in the subregion of U lying east of the W. 89° meridian. We shall denote this subregion by A. In view of its relatively small size, one might expect discovery conditions within it to be sufficiently uniform to insure that hypothesis H is satisfied inside its boundaries. Actually, as is shown by Table 3 (*in which the grouping necessitated by the large number of values of N tends to improve the agreement between the values of* $\nu_c(j)$ *and* $\nu_o(j)$), the values of $\nu(j)$ for A, computed from (2), with $n_A = 9$ and $N_A = 95$, differ greatly from the observed values of this quantity, particularly for $j = 0$ and for $j \geqslant 21$.

TABLE 3

j	0	$1 \leqslant j \leqslant 5$	$6 \leqslant j \leqslant 10$	$11 \leqslant j \leqslant 15$	$16 \leqslant j \leqslant 20$	$21 \leqslant j$
$\nu_c(j)$	<0.0003	0.4	4.2	3.7	0.6	<0.06
$\nu_o(j)$	1.	2.	2.	1.	2.	1.

In view of this result, it appears almost certain that something besides mere chance functions to produce the observed distribution of meteorites in A. We shall return to this question in the next section.

The portion of U remaining after A is deleted consists of 35 "squares" containing 66 recognized meteorites. As in the case of R as a whole, one would judge it very unlikely that the hypothesis H is fulfilled, even roughly, throughout the vast extent of the region U-A. Calculations employing (2), with $n = 35$, $N = 66$, speedily confirm this opinion. Closer examination of U-A reveals that, like R, it sub-

divides naturally into 2 parts; a Great Plains province, *B*, and a Western province, *C*. The region *B* extends from the Gulf to Canada and is made up of States characterized by climatic similarities and flat, treeless, prairie land containing in general few rocks and very extensively cultivated. Moreover, the States of this province all have approximately the same moderate population-density and have been settled for about the same length of time. The province *C* extends from the Continental Divide to the Pacific coast and is characterized by vast expanses of rocky mountains and deserts, all prospected to about the same extent by several generations of miners. Except for a narrow strip along the coast, all parts of this province present certain climatic similarities, support approximately the same scanty populations, and have been inhabited by white men for the same length of time. One would therefore anticipate that of all the areas studied, *B* and *C*, taken individually, are the ones in which the hypothesis *H* is most nearly satisfied. If we test this conclusion by calculation, using the values $n_B = 14$, $N_B = 41$, $n_C = 21$, $N_C = 25$, deducible from Fig. 2, we are led to the results exhibited in Tables 4 and 5, following:

TABLE 4

j	0	1	2	3	$\geqslant 4$
$\nu_c(j)$	0.7	2.1	3.1	2.9	4.3
$\nu_o(j)$	4.	1.	3.	2.	4.

TABLE 5

j	0	1	2	3	$\geqslant 4$
$\nu_c(j)$	6.4	7.6	4.5	1.8	0.7
$\nu_o(j)$	9.	10.	3.	1.	2.

The agreement between the calculated and observed values of $\nu(j)$ is, on the whole, quite satisfactory and leaves little doubt that in each of the regions under consideration the observed distribution of meteorites is essentially the result of chance.

NOTES AND REFERENCES

[1] In what follows, we shall frequently omit the word "recognized" where no confusion can occur. A recognized meteorite may be one recovered from a witnessed fall or simply a find.

[2] Farrington, O. C., *Pop. Sci. Monthly*, **64**, 351-4, 1904.

[3] Fisher, W. J., *P. A.*, **42**, 501-4, 1934.

[4] Farrington, O. C., *Mem. Nat. Acad. Sci.*, **13**, 11, 1915.

[5] Olivier, C. P., *Meteors*, 239-40, 1925.

[6] *Cf.* Melton, F. A., and Schriever, William, *Jour. Geol.*, **41**, 52, 1933, and Wylie, C. C., *Univ. of Iowa Obs., Contr. No. 4*, 140, 1933; *P. A.*, **41**, 410, 1933.

[7] Our choice of material is governed by the fact that Farrington systematically included the geographical coördinates of the places of find of the meteorites cataloged by him. From the viewpoint of the present paper, such coördinate data are essential. Their omission precludes use of certain catalogs issued since the appearance of Farrington's *Memoir, e.g.*, the otherwise valuable *Catalogue* (1923) and *Appendix* (1927) of G. T. Prior.

[8] A more precise description of the boundaries of the region denoted by *R* is given later.

[9] A point will be said to be taken at random in *R* if it is so placed that subregions of *R* of equal area are equally likely to contain it.

[10] Consult Poincaré, H., *Calcul des Probabilités*, Chap. IV, Paris, 1912.

[11] *Cf., e.g.*, Uspensky, J. V., *Introduction to Mathematical Probability*, Chap. VII, New York, 1937.

[12] Thus, in our case, the average values, $\bar{\nu}_o(j)$, approach more closely to the theoretical values, $\nu_c(j)$, than is true of either the first or the second set of $\nu_o(j)$-values.

[13] The Lake Okeechobee, Florida, stone appears to be the only one of the recognized meteorites of the world of which all recovered fragments were found by accident in a water area.

[14] This particular discrepancy has its origin in the southern Appalachian meteoritic concentration.

[*To be continued.*]

An Observation of a Peculiar Meteor Train

By ROBERT LEIGHTON *and* CHARLES H. WILTS

An observation of a peculiar meteor train was made at $22^h\,05^m$ P.S.T., on December 15, 1939, with an 8-inch telescope, from a point 5 miles due north of the Mount Palomar Observatory, San Diego County, California. The magnitude of the meteor appeared to be about —5 and its course started about 2° or 3° N. of α Canis Minoris (Procyon) and ended about 2° E. of β Aurigae. The meteor traversed its path in about 2^s and left a naked-eye train over its entire path; this naked-eye train lasted about 5^s-10^s and its initial brightness appeared to be somewhat greater than the earth-light on the new moon. Its width appeared to be greater near the middle of the train, but this effect may have been due to greater brightness in that region. The head of the meteor was a pale orange, becoming a pale blue about 15′ behind the head. The naked-eye train appeared white. Luckily we were using a low power (×50) in the observation of some clusters and nebulae, rendering it very easy to pick up the train in the telescope. The central region of the train was observed, and it at first appeared somewhat brighter than the Orion nebula, becoming very difficult to observe after approximately 60^s. The first view revealed not a single train, but *two* well-defined, perfectly parallel trains of equal brightness, about 5′ apart, each train being about 1′ in width. Sweeping along the trains brought into view an abrupt, *S*-shaped bend, after which the trains continued parallel to their original course but displaced laterally about 20′. Just before and after the bend, the two trains lost their well-defined quality, having a mottled appearance and being rather disconnected. The trains were unaffected by this abrupt displacement, remaining the same in brightness, width, and definition after the bend. This singularity was observed about 15^s-20^s after the meteor had passed, and did not change its form perceptibly in the 45^s during which we observed it; these facts should exclude air currents as a cause. We failed to look for possible movement of the train as a whole. On the bend itself, the trains were well-defined but not quite parallel. The angle through which the trains were bent was very nearly a right angle; Leighton thinks it was slightly greater, but Wilts remembers it as being slightly less. Both observers agree, however, that it was within 5° of a right angle. As the double train was becoming faint, Leighton swept along it for at least 2° on each side of the bend, but could detect no irregularities other than an occasional small condensation in one or the other of the components. Because he was locating the train in the telescopic field of view, he had no opportunity to study it in detail with the naked eye, but Wilts and a third observer, Robert Krueger, agree that the naked-eye train had no visible irregularities in its entire length. Krueger was not very well acquainted with the telescope or with the sky, and consequently felt that he could give no quantitative description of what he saw; the preceding estimates are, therefore, largely the result of the impressions received by the writers. In all cases, they were in very close agreement.

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VARIABLE STARS

Variable Star Notes from the American Association of Variable Star Observers

By LEON CAMPBELL, Recorder

Variation of Periods in Eclipsing Variables: A recent *Contribution* from Princeton Observatory contains a very important study, made by R. S. Dugan and Miss Frances W. Wright, of the variation in period of certain eclipsing variables. The authors show that the problem of eclipsing variables is not a simple geometrical proposition, as was once believed: the prototype, Algol, has shown a puzzling variation in period ever since its discovery. Whereas in early years the main purpose of observers was to determine times of minima and calculate mean periods (which required only fairly accurate data), in more recent years the need for observations of a high degree of accuracy has been felt, and in fact demonstrated. Such observations have been made by experts with photometers capable of determining light curves with great accuracy; and these curves have revealed facts truly surprising which have enabled the theorist to determine many properties concerning eclipsing stars: the shapes, sizes, and masses of the components, as well as many other characteristics.

One of the advantages of a collection of photographic plates covering many years, and especially the years preceding the actual discoveries of the stars in question, is that on these plates are observational data which not only supply hitherto unknown facts, but also suggest plans for future observing programs.

The recent study of eclipsing variables by Dugan and Wright from Harvard plates has been particularly valuable in furnishing strong evidence of changes in period. The observational material was secured by Miss Wright, working at the Harvard Observatory, and included twenty-eight variables. Of these, seventeen showed variation in period; seven had periods probably constant; three, periods perhaps variable; and one, probably variable.

Details regarding each of the variables are plainly set forth, and not only are the "current elements" given, but also, when found, time-residuals showing the deviation from uniform periods. Considerable published data on the same stars are included in the discussions.

Rotation of the line of apsides, as well as orbital motion about a third body, have frequently been assigned to explain the changes in period among eclipsing binaries. The slowness with which these changes take place makes the whole problem a difficult one, and therefore even fragmentary evidence must be tentatively accepted. It is pointed out that, when all eclipsing variables are considered, those with apparent apsidal motion are few in number, the chief reason being that most of their orbits are circular. There is no clear indisputable evidence, among the stars here studied, of motion about a third body.

Several of the variables supply examples of the queer, unpredictable type of residual curves. No case appears in which a parabolic term is substantiated. Long continued observations show that when a parabola is at first indicated, then rather suddenly the curve deviates in an unpredicted direction. There is little justification for sine curves, and therefore one must keep linear elements up to date for any accurate prediction.

The investigation opens up a field of research in which many possibilities arise.

Residual curves are varied in form, as is clearly shown in the diagrams presented. They recall many similar curves found among other types of variation. It is a piece of work well done and deserving of great commendation.

Visual Light Curve of β Doradus: The Cepheid variable β Doradus was at one time considered a pseudo-Cepheid, or a Cepheid with small variation in light. In 1927 Shapley and Walton derived a photographic light curve from Harvard plates, with an indicated range in magnitude from 4.25 to 5.65 in a period of 9.8417 days (see *H.C.* 313, 1937). Since then the star has been considered a typical Cepheid with ζ Geminorum characteristics.

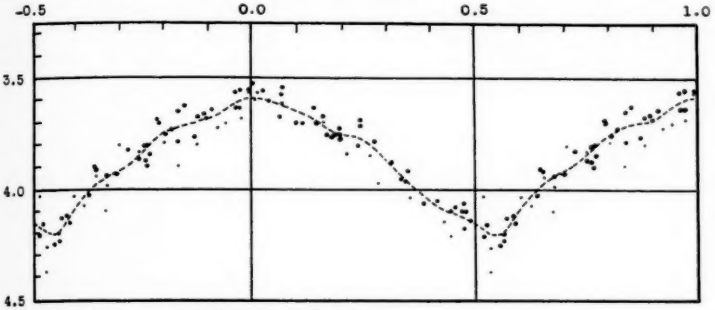

FIGURE 1.– OBSERVATIONS OF β DORADUS.

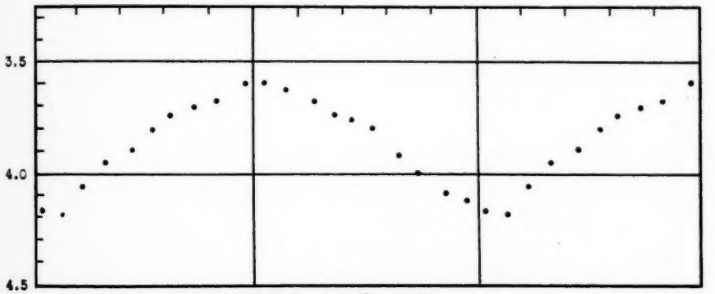

FIGURE 2.– NORMAL MAGNITUDES OF β DORADUS

A recent communication from A. W. J. Cousins of Durban, South Africa, gives what appears to be the first visual light curve of this star, resembling in many of its details the form of the Harvard photographic light curve. The visual range is from $3^M.6$ to $4^M.2$, or $0^M.6$, as compared with $1^M.4$ for the photographic range. How much of this difference in range may be due to the adopted scale of the magnitudes is still a question, but that there is a marked difference is undeniable. If the visual curve is plotted on a scale of one magnitude equal five inches, and the photographic curve, one magnitude equals two inches, the two curves are in remarkable agreement. Figure 1 represents the individual observations by Cousins, and Figure 2, the normals, or mean magnitudes.

Dates of Maxima of Bright Long-Period Variables, 1940: Of the hundreds of long-period variables under regular observation by the A.A.V.S.O., a score or more attain a maximum brightness well above the eighth magnitude. Dates dur-

ing 1940 when the latter are due to reach their maxima are listed herewith, together with their approximate magnitudes at those times. The vagaries of the stars themselves render the values somewhat uncertain (see mention of *o* Ceti and χ Cygni in earlier "Variable Star Notes"). Dates of predicted maxima can be depended upon to within ten or twelve days, if we judge by predictions of previous years.

Design.	Name	Date of max.	Approx. mag.	Design.	Name	Date of max.	Approx. mag.
0010*32*	S Scl	Dec. 12	6	153656	T Nor	Jun. 26	7
0214*03*	*o* Cet	Jul. 8	3	154615	R Ser	Oct. 5	7
023133	R Tri	Apr. 13	6	154639	V CrB	Jul. 22	7
0250*50*	R Hor	Feb. 25	6	162119	U Her	Nov. 21	7
070122a	R Gem	Jan. 31	7	163266	R Dra	May 12	7
081112	R Cnc	May 21	7	164715	S Her	Oct. 29	7
0929*62*	R Car	Nov. 17	4	1648*44*	RS Sco	Dec. 9	7
093934	R LMi	Jul. 21	7	1650*30*	RR Sco	Jun. 2	6
094211	R Leo	Aug. 12	5	1702*15*	R Oph	May 1	7
1006*61*	S Car	Jul. 27	6	183308	X Oph	Jul. 2	7
103769	R UMa	Sep. 11	7	190108	R Aql	Oct. 14	6
123307	R Vir	May 4 / Oct. 8	7	1910*19*	R Sgr	Oct. 26	7
1327*06*	S Vir	Dec. 30	7	194632	χ Cyg	Oct. 3	5
1409*59*	R Sen	Mar. 25	6	200938	RS Cyg	Mar. 24	7
143227	R Boo	Jun. 1	7	2011*29*	RT Sgr	Aug. 18	7
151731	S CrB	Sep. 4	7	210868	T Cep	Oct. 3	5
1528*49*	R Nor	Mar. 3 / Aug. 14	7	235350	R Cas	Oct. 14	6

Harvard's Ten-Thousandth Variable: Harvard Variable 10,000, discovered on July 18, 1939, by Miss Cannon, lies in the rich region of Sagittarius at R.A. $18^h\ 06^m\ 48^s$, Decl. −30° 17′.6 (1900), and proves to be an interesting, if not a perfectly satisfactory, sort of variable star. Miss Swope has measured the star on numerous Harvard plates and finds that the range is at least three magnitudes, perhaps much more, and that there appears to be a suggestion of a period of about 800 days; the star may therefore be listed among the semi-regular variables.

It is of interest to note that Harvard's No. 1 variable is the well-known, and now well-observed, star T Orionis, situated in the outskirts of the Great Orion Nebula, and confirmed as a variable by Professor G. P. Bond in 1863. Harvard Variables Nos. 1 and 10,000 have some similar aspects in light variation: both are subject to marked irregularities, and both, at times, present suggestions of periodicity, No. 1 in 80 and 160 days, and No. 10,000, in about 800 days.

If the variation of H.V. No. 1, T Orionis, can be explained, in part at least, by association with a bright gaseous nebulosity, may it not be possible that H.V. 10,000 is involved in a dark, obscuring cloud which accounts for some of its variation in light?

Nearly twenty years after the discovery of T Orionis, Professor E. C. Pickering, from a visual examination of their spectra, found two other variables, U Puppis, H.V. No. 2, and R Ursae Minoris, H.V. No. 3, both long-period variables.

From a study of photographed spectra of class M stars, in which bright hydrogen lines predominate, Mrs. Fleming found between the years 1889 and 1902 a majority of Harvard's first 200 variables. Miss L. D. Wells counted among her discoveries the familiar variable star SS Cygni, (H.V. 84), which has been so assiduously observed during the past 40 years or more.

In 1901 Professor Bailey greatly increased the number of Harvard Variables

through his explorations in globular clusters, so that at the end of that year more than 700 variables had been discovered at Harvard. At the end of 1910 the number had increased to 3306, the growth in the discoveries of the first decade of this century resulting from an intensive search, mainly by Miss Leavitt, in the Magellanic Clouds and some rich regions in the Milky Way.

Since 1910 the rate of discovery of variable stars has been steadily progressing until now it is safe to say that more than half of all known variables have been found on plates at Harvard; doubtless these same plates contain enough undiscovered variables to swell the total to over thirty thousand. Harvard Variable No. 10,000 marks an epoch in variable star discovery, and this particular star deserves still further observational attention, mainly because of its apparent peculiarities.

Observations Received During January, 1940:

Observer	Var.	Obs.	Observer	Var.	Obs.
Albrecht	10	10	Irland	6	8
Baldwin	89	139	Jones	65	165
Ball, A. R.	21	21	Kearons, Mrs.	55	93
Ball, J.	31	31	Kelly	12	24
Bappu	35	132	Kirkpatrick	29	71
Blunck	10	10	de Kock	51	221
Bouton	33	47	Loreta	153	440
Britzelmayer	3	5	Lovinus	2	2
Buckstaff	13	48	Lundquist	19	47
Callum	28	28	Maupomé	34	38
Chandra	212	410	McKeon	1	1
Cilley	24	49	McPherson	17	17
Cousins	28	91	Moore	4	5
Dafter, Mrs.	8	23	Palo	13	28
Diedrich	8	8	Parker	16	20
Dougherty	2	2	Peltier	13	63
Ensor	50	86	Prinslow	2	2
Escalante	100	215	Purdy	10	10
Ferguson	7	10	Rense	49	49
Fernald	54	75	Roe	2	2
Ford	11	11	de Roy	17	61
Gregory	45	50	Schoenke	3	5
Griffin	18	18	Schmid	25	25
Guthrie	2	2	Sill	13	13
Halbach	67	73	Smith, F. P.	11	17
Harris	36	38	Stahr	5	5
Hartmann	139	159	Topham	20	21
Heckenkamp	5	8	Treadwell	11	16
Heidelberg	2	2	Webb	17	17
Holmes, Mrs.	7	7	Weber	45	45
Holt	114	304	Williams	10	31
Houghton	78	106	Yamada	10	46
Howarth	20	28			
				65	3864

February 12, 1940.

Notes from Amateurs

The Joliet Astronomical Society

The Joliet Astronomical Society began its fall schedule October 3 with an outdoor camp-fire supper sponsored by the sister organization, the Joliet Mineralogists, at the Council Circle in Pilcher Arboretum. At 7:30 the group adjourned

to Little Palomar, new observing site where President Walter B. Huey, Professor Clarence R. Smith, and Mr. Paul Ohman had their telescopes set up on new concrete bases and all had views of the planets and other objects of interest. An aurora beginning at about 7:30 continued to grow in brilliance and beauty, passed through many stages culminating in the formation of a huge bow starting in the east, passing through the zenith, and finally reaching into the haze in the west toward the city.

On October 10 the election of officers took place with Dr. Huey remaining as president and Robert L. Price, Ben Hur Wilson, and Paul Ohman acting as vice-presidents with various duties. Chester Moore was elected secretary and Miss Mary Fant, treasurer.

Mr. R. D. Cooke, formerly of Milwaukee, gave an illustrated talk on the building of the observatory of the Milwaukee Astronomical Society, at this meeting. Members discussed the August auroras viewed by many of them from various parts of the country.

The meeting on October 24 was devoted entirely to phases of amateur astronomy. On November 7 the society held an exhibit and Robert L. Price entertained many guests in the Junior College observatory as a part of the observance of National Education Week by the Joliet Township High School and Junior College.

The winter series of talks by members of the Joliet Astronomical Society began with a sketch of the life of A. A. Michelson, "The Man who Measured the Stars" by Professor Clarence R. Smith of Aurora College who had collected much material on this subject. An interesting travelogue presented by Mr. and Mrs. Frank M. Preucil who had visited the meteor craters and observatories of the Southwest was illustrated with colored stills and colored movies. A discussion of the theories of the origin of the solar system was Leonard Onsgard's contribution and a review of Professor Baker's book, "Introducing the Constellations" was given by Mr. Price.

The annual Christmas tea was held in the spacious home of Dr. and Mrs. Walter B. Huey and the program of the evening consisted of a short concert of carols by the Madrigal Singers of the high school and readings of astronomical poetry by the members. Refreshments were served from a beautifully appointed table with Mrs. Everett C. Shaw and Mrs. Ben Hur Wilson pouring.

An outstanding event was the visit and lecture by Professor E. A. Fath of Goodsell Observatory of Carleton College on January 2. Professor Fath took his audience on a tour of the western observatories and illustrated his interesting account with fine kodachrome slides. He was introduced by Superintendent C. L. Jordan, new head of the Joliet Township High School and Junior College.

Due to illness, Chester Moore has resigned as secretary and the office is being filled by the undersigned.

MONICA M. PRICE, *Secretary.*

The Cleveland Astronomical Society

At the Warner and Swasey Observatory Friday, January 26, 1940, we had our second annual lecture on "Astronomical Progress" during 1939. Dr. J. J. Nassau, Director of the Observatory, was our speaker. The following were listed as the significant achievements in astronomy during the past year.

1. The opening of the MacDonald Observatory and the scientific contributions at the time of the dedication.
2. The generation of energy within the stars and stellar evolution as investigated.

3. The detection of the corona by means of the photocell.
4. The determination of the orbit of Neptune and the mass of Pluto.
5. The nature of supernovae.
6. The recent approach of Mars and the photographic studies of the planet by Dr. E. C. Slipher.
7. The projected large Schmidt telescopes.

Plans for the enlarging of the present observatory were discussed. Work is to begin immediately and it is hoped that the building operations can be completed by June. The new big telescope will be a memorial to the late Edward P. Burrell, for many years the director of engineering of the Warner & Swasey Company, and the designer of many big telescopes built by the company. The new telescope will be a new Schmidt type with 36-inch mirror and 24-inch lens. It will employ a 9-inch sighting telescope or "finder." The new dome will be 28 feet in diameter. A large room will be available for meetings of our society.

DON H. JOHNSTON, *Recording Secretary.*

Euclid Beach Park, Cleveland, Ohio.

Solar Photography with a Ninety-Cent Camera

The joys which an amateur astronomer experiences in the solutions of the problems which lie within the scope of his endeavors are great, but the greater joy comes in being able to pass along the results of his experiments so that other amateurs may share, too. It is with this thought in mind that the author desires that this article should be considered as a provisional report in the hope that other amateurs, whose endeavors lie in different fields of observational astronomy, will take up these results and apply them, be it in stellar, lunar, meteoric, the planet, or the aurora studies.

The limited means of the amateur does not always deprive him of the necessary instrumentation and material he is in need of to study the subject which lies so close to his heart. Somehow or other with the material at hand, he molds, shapes and grinds until he holds in his hands a substitute which serves him well.

Photography of this sort can be applied to either the reflector or the refractor type telescope which the amateur has at his command. In the case of the author it is a Mogey refractor type, of three inches aperture, forty-one inches focal length, equipped with a Mogey solar-prism and an eyepiece of moderate power giving a substantial field. The assembly can be seen on the accompanying Figure. A block of aluminum was placed in the lathe and a hole was bored so as to fit the draw tube of the telescope, the block was then drilled and tapped. For the two holes on the bottom of the block two screws were made with knurled control. For the hole on the top, a shaft with sufficient threading for height adjustment was made, which turns through a knurled nut for locking when once the necessary height is attanied. The block was then sawed through to allow the assembly of it on the draw tube and the walls of the large hole were lined with an adhesive tape so as not to mar the tube and to allow a tighter fit. This assembly is very rigid which is an essential factor. The frame or bracket on the camera and the focusing tube are parts of a B & L Microphotography outfit. There are two knurled screws for securing the assembly to the shaft. On the lathe was also turned a hard-wood collar, made so as to prohibit any light leak. The bottom was fitted snugly over the eyepiece and the top to receive the camera's cell containing the lens.

The clamp as shown on the photograph, below the wooden collar, is a reel-clamp for deep sea fishing and is here used to keep the eyepiece stationary when

once the proper focus is obtained.

A portrait lens is fastened on the optical system of the camera for better definition at close range and the camera itself is equipped with proper automatic timing. The cost of the portrait lens was from fifty to seventy-five cents.

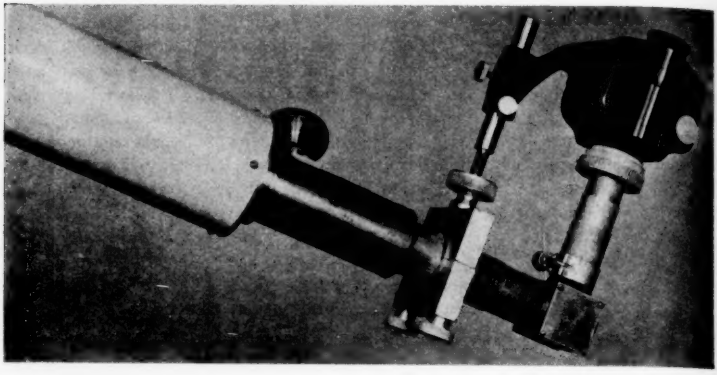

FIGURE 1

The Mogey solar prism is equipped with a lens to let out the heat from the sun and this was used for guiding as the image can be localized on this lens.

Eastman Panotomic film was used on Agfa, Fino-pan because of its fine grain which permits better enlargement.

It is hoped that amateurs interested in this experiment will convey their results to the author who will welcome advice and criticism to perfect this venture. Suggestions from professional photographers will be greatly appreciated because of the author's limited knowledge of photography.

NEAL J. HEINES.

560 Broadway, Paterson, New Jersey, August 19, 1939.

General Notes

Dr. Edwin P. Hubble of the Mount Wilson Observatory has been awarded the Gold Medal of the Royal Astronomical Society, London, "for outstanding work on distances, velocities, distribution and nature of extra-galactic nebulae." (*Science*, January 26, 1940)

The Royal Astronomical Society of Canada at the annual meeting held at the University of Toronto on January 16 elected Dr. J. A. Pearce, assistant director of the Dominion Astrophysical Observatory, Victoria, B. C., president for 1940. Dr. Frank S. Hogg, of the David Dunlap Observatory of the University of Toronto, and Dr. A. Vibert Douglas, of the department of physics of McGill University, were elected vice-presidents. E. J. A. Kennedy, of Toronto, was re-elected general secretary; J. H. Horning, of Toronto, treasurer, and Dr. Peter M. Millman, of Toronto, librarian. Dr. William Findlay, professor of mathematics at McMaster University, Hamilton, Ontario, delivered the address of the retiring president

before the society at the annual "at-home" held in the McLennan Physics Laboratory of the University of Toronto on January 26. He spoke on the year's achievements in astronomy and allied sciences. (*Science*, January 26, 1940)

The Evening Sky

On this clear evening, at about 6:30, a low arch of the aurora shone faintly in the north. Orion approached the south. The Milky Way, overhead, spanned the sky from southeast to northwest. Near the vernal equinox a slender triangle, Venus and the new moon—with the ghost of the old—under Jupiter, pointed upward to Saturn and Mars. One might not see such a sight again in a lifetime.

Centerville, Massachusetts, Feb. 11, 1940. JOSEPH DWIGHT.

New Double Star

On 1940 January 3, while looking for the double ADS 2689, I ran across another whose components I estimated at 8.8 and 9.2 magnitudes. Measures gave $p = 213°.2$ and $d = 2''.73$. As the star is fairly bright, I was surprised to find it was not given in the catalogue, and a letter to Dr. R. G. Aitken confirms that no one had reported it since. It has now been measured on five nights by Dr. S. G. Barton, whose mean results are: $p = 218°.4$ and $d = 2''.80$. The star is BD—3°579, magn. 9.2. I think that the B.D. estimate is about half a magnitude too faint.

Flower Observatory, 1940 January 25. CHARLES P. OLIVIER.

Book Review

Stars of Spring and Summer, and Stars of Fall and Winter, by Theodore G. Mehlin. (University Press, 1066 U. P. Station, Des Moines, Iowa. $1.95 for each section.)

Dr. Mehlin has compiled an attractive set of charts for beginners who wish to study the constellations. There are two sets of charts, called, respectively, *Stars of Spring and Summer*, and *Stars of Fall and Winter*, the two sets together containing a total of 48 charts. The individual charts show the stars as white dots against a black background, and are made on stiff paper 8¾x11½ inches in size, suitable for outdoor use at night. Each set contains 12 charts depicting the relative configurations of constellations appearing in the north, east, south, and west quadrants of the sky, for every month of the six months period indicated by the title. In each set are also to be found 12 additional charts portraying individual constellations. These are drawn to a larger scale, and show, for each constellation portrayed, the outline of the mythological figure associated therewith. There is, on the back of each, a description of the mythological legend associated with that constellation.

These charts are desirable for high school or grade school groups, scouts, or college students who may be taking up constellation study. One set of charts should prove sufficient for a number of students, since there are separate charts for each quadrant of the sky, and again for each constellation. R.S.Z.

Carleton College, Northfield, Minnesota.

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